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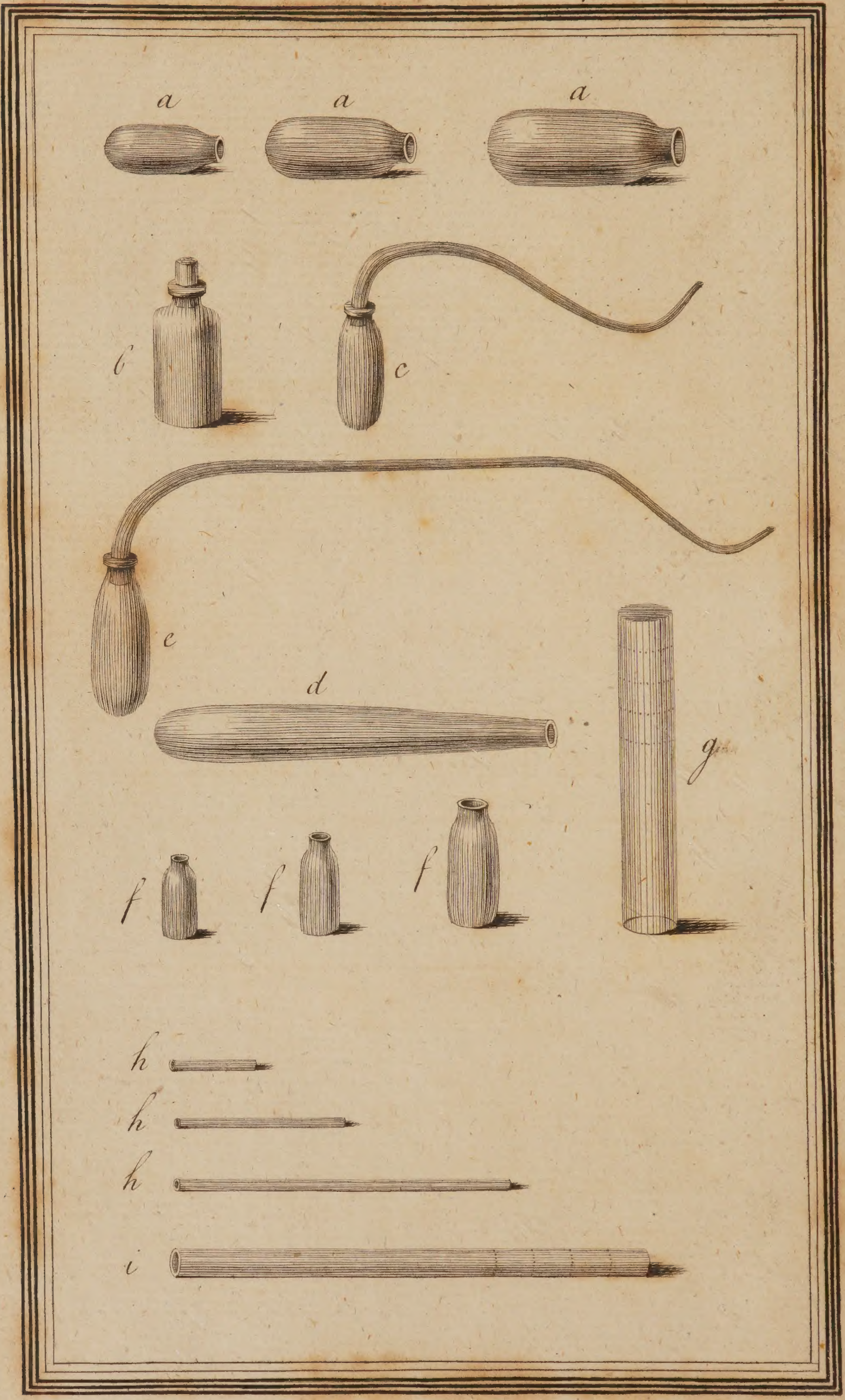

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EXPERIMENTS

AND

OBSERVATIONS

ON DIFFERENT KINDS OF

AIR.

VOL. II.

By JOSEPH PRIESTLEY, LL. D. F. R. S.

Ita res accendunt lumina rebus.
LUCRETIUS.

THE SECOND EDITION.

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TO

Sir JOHN PRINGLE, Bart.

PRESIDENT OF THE ROYAL SOCIETY,

THIS SECOND VOLUME OF

EXPERIMENTS AND OBSERVATIONS,

ON A SUBJECT TO WHICH HE HAS GIVEN
PARTICULAR ATTENTION,

AND IN THE INVESTIGATION OF WHICH HE
HAS PUBLICKLY AND PRIVATELY ENCOURAGED THE AUTHOR,

IS,

WITH THE GREATEST RESPECT, INSCRIBED,

BY HIS OBLIGED,

HUMBLE SERVANT,

London, Nov. 1775. J. PRIESTLEY.

THE

PREFACE.

I HAVE ſeen abundant reaſon, ſince the publication of my former volume of *Obſervations on different kinds of air*, to applaud myſelf for the little delay I made in putting it to the preſs; the conſequence having been that, inſtead of the experiments being proſecuted by myſelf only, or a few others, the ſubject has now gained almoſt univerſal attention among philoſophers, in every part of Europe. In conſequence of this, conſiderable diſcoveries have been made by people of diſtant nations; and this branch of ſcience, of which nothing, in a manner, was known till very lately indeed, now bids fair to be farther advanced than any other in the whole compaſs of natural philoſophy. The attention which my former volume excited, has been a motive with me to continue my own reſearches, till I have been led to diſcoveries of more importance than any that I had made before, and of which I had not at that time the moſt diſtant idea. It has, likewiſe, been the means of extending my acquaintance among philoſophers, of whoſe lights I have availed myſelf, as my narrative will witneſs.

Sig. Felice Fontana of Florence, Sig. Landriani of Milan, and Mr. Lavoiſier of Paris, have already announced, in their late publications on this ſubject, that they have much more to follow with reſpect to it, and that they are at preſent intent upon the inveſtigation of it. Mr. Montigny (when I had the pleaſure of meeting him at Mr. Trudaine's, as mentioned in the courſe of the work) gave me an account of ſome very curious experiments which he had made on inflammable air, and which I expect he will ſoon communicate to the public. That veteran in philoſophy, Father Beccaria of Turin, has alſo made ſome valuable experiments of this kind, and will, I doubt not, proſecute them with his uſual addreſs and ſucceſs. Mr. Bergman of Upſal, who, as I have ſaid, wrote to me formerly on this ſubject, has ſince publiſhed a paper on fixed air in the Swediſh language, which I cannot read. Several ingenious perſons, whoſe names are not yet known to the public, are, to my knowledge, engaged in theſe purſuits; and we are not without expectations from the oldeſt *living fathers* of this philoſophy, Dr. Brownrigg and Dr. Black, as well as other gentlemen in Scotland. Beſides theſe, there muſt be, I doubt not, at leaſt twice as many perſons at work upon this ſubject, as I can have had any opportunity of hearing of.

Upon the whole, there is not perhaps an example, in all the hiſtory of philoſophy, of ſo much zeal and emulation being excited by any object. I even queſtion whether the ſubject of *electricity*, under the auſpices of Dr. Franklin, ever engaged more general attention; and now theſe two purſuits are happily united, and admirably promote each other.

In reality, this is not now a buſineſs of *air* only, as it was at the firſt; but appears to be of much greater magnitude and extent, ſo as to diffuſe light upon the moſt *general principles* of natural knowledge, and eſpecially thoſe about which *chymiſtry* is particularly converſant. And it will not now be thought very aſſuming to ſay, that, by working in a tub of water, or a baſon of quickſilver, we may perhaps diſcover principles of more extenſive influence than even that of *gravity* itſelf, the diſcovery of which, in its full extent, contributed ſo much to immortalize the name of Newton.

Having been the means of bringing ſo many champions into the field, I ſhall with peculiar pleaſure attend to all their atchievements, in order to prepare myſelf, as I promiſed in the preface to my laſt volume, for writing the *hiſtory* of the campaign; and I truſt that all my brethren in the ſcience will have confidence in my juſtice to their reſpective merits.

I flatter myſelf, that the very frank and candid manner in which I have related what I have done myſelf, will procure me ſufficient credit for my impartiality with reſpect to others. It will be very evident, that I have left myſelf hardly any other merit than that of *patient induſtry* and *attention*, and that of keeping my mind ſo far detached from the influence of prejudice, as to be able to purſue fairly ſuch caſual obſervations as preſented themſelves to me.

There is nothing capital in this volume from which I can hope to derive any other kind of honour, than that of being the inſtrument in the hands of divine providence, which makes uſe of human induſtry to ſtrike out, and diffuſe, that knowledge of the ſyſtem of nature, which ſeems, for ſome great purpoſe that we cannot as yet fully comprehend, to have been reſerved for this age of the world; concerning which I threw out ſome farther hints in my former preface, which the excellent French tranſlator was not permitted to inſert in his verſion.

I even think that I may flatter myſelf ſo much, if it be any flattery, as to ſay, that there is no hiſtory of experiments more truly *ingenuous* than mine, and eſpecially the Section on the diſcovery of dephlogiſticated air, which is the moſt important in the volume. I am not conſcious to myſelf of having concealed the leaſt hint that was ſuggeſted to me by any perſon whatever, any kind of aſſiſtance that has been given me, or any views or hypotheſes by which the experiments were directed, whether they were verified by the reſult, or not.

In this volume the reader will find much light thrown upon many things which were inexplicable to me when I publiſhed the former volume; but, on the other hand, there are many things, in this, as inexplicable to me now as the others were before; and for the elucidation of them we muſt wait for more experiments, and more diſcoveries.

As, in the preface to my former volume, I quoted a very ſtriking obſervation of Father Beccaria, I ſhall, in this, preſent my reader with a quotation from another Italian philoſopher, the Abbe Fontana, which is as much to my preſent purpoſe.

“ Le ſiſiche ricerche cominciate in queſti
“ ultimi anni con tanto ſucceſſo dai filo-
“ ſofi, forſe per mera curioſità, ſopra le
“ diverſe qualità e indole dell' aria natu-
“ rale e fattizia, potrebbero in breve diven-
“ tare di ſomma importanza. E par' che
“ già ci avviciniamo ad una di quelle gran-
“ di epoche, che la natura conduce, dopo
“ un laſſo di ſecoli, e che marca con qual-
“ che grande ſcoperta, per la felicità del
“ genere umano.” *Ricerche Fiſiche*, p. 21.

In Engliſh.

“ The inquiries that have lately been
“ ſo ſucceſsfully begun by philoſophers,
“ perhaps through mere curioſity, into the
“ properties of air, natural and facti-
“ tious, may ſoon come to be of the
“ greateſt importance. And we ſeem to
“ be already approaching to one of thoſe
“ great epocha's, to which nature con-
“ ducts us, after a lapſe of ages, and
“ which ſhe diſtinguiſhes by ſome great
“ diſcovery, for the benefit of mankind.”

To this ſecond volume I have added a paper publiſhed in the 60th volume of the Philoſophical Tranſactions, on the *conducting power of charcoal*, becauſe it has a near relation to the ſubject of *air*; and becauſe it contains an account of many new facts, of a very remarkable nature, to which I wiſh to draw the particular attention of philoſophers and chymiſts.

I have alſo inſerted the ſubſtance of the pamphlet on *the impregnation of water with fixed air*, having no intention to publiſh it any more ſeparately; prefixing to it a hiſtory of matters relating to it, and ſubjoining to it a compariſon of this method with another that has been invented ſince, for the ſame purpoſe. I have alſo added an *alphabetical index* to both the volumes.

I am very ſorry to have had occaſion to inſert in this volume a particular ſection on the *miſtakes* that have been made, with reſpect to my Obſervations and Experiments, by ſeveral foreign philoſophers. But they are ſo many, and ſo groſs, and made by perſons of ſo much reputation, that I have thought it neceſſary to do ſo, both on my own account, and alſo to obviate ſuch miſrepreſentations of *facts*, as might retard the progreſs of philoſophical knowledge.

For theſe miſtakes foreigners may plead the want of a perfect knowledge of the Engliſh language; and, in ſome meaſure, the plea may be admitted, though every perſon ſhould take care to make himſelf fully acquainted with what he propoſes not only to underſtand, but to explain to others.

I imagine, however, that both Mr. Lavoiſier, and Sig. Landriani, took their accounts of my Experiments not from my own work in Engliſh, but from ſome preceding tranſlations into French, and Italian; taking for granted that they were exact. Sig. Landriani, I am confident, underſtands Engliſh very well. He has lately informed me, that he will undertake the tranſlation of all that I have written on the ſubject of air; and he will, I doubt not, both do juſtice to me, and credit to himſelf. Mr. Gibelin, who has acquitted himſelf ſo well in the French tranſlation of the firſt volume of this work, has undertaken the ſecond. I have, at his requeſt, already ſent him the printed ſheets, and I believe he will diſpatch the work with all convenient expedition. I have alſo reaſon to think, that the tranſlation of this work into High Dutch, by Dr. Ludewig of Leipſic, will be very accurate. Upon the whole, therefore, I flatter myſelf that, for the future, my ſenſe will be fairly repreſented; and perhaps with more accuracy, than if the miſtakes I have been obliged to animadvert upon had not been made.

I wiſh that I could make as good an apology for Mr. B. Wilſon, as for the foreign philoſophers above-mentioned. This gentleman, in his late *Treatiſe on phoſphori*, has animadverted upon me for ſpeaking, in my *Hiſtory of diſcoveries relating to viſion, light, and colours*, of paper being *red hot*, and *cooled again*; when, in the printed *errata* of the book, he would have found, "for *red hot*, read *very hot*." This was a miſtake of my amanuenſis, and I thought it would be ſufficienlty rectified, by inſerting it in the *errata*. I ſhould certainly have reprinted the leaf, if I could have ſuſpected that ſuch an uſe would have been made of it. Before this gentleman points out any more of my miſtakes, I hope he will take the trouble to ſee whether I have not noted them myſelf; or, if his copy of my books ſhould happen to want the printed *errata*, that he will ſupply the place of it with a little *candour*. His paragraph relating to it, p. 10, is as follows.

"Dr. Prieſtley, in his *Hiſtory of Beccari's diſcoveries*, has mentioned a very "remarkable experiment. He tells us, "that Beccari found that paper, after it "had been made *red hot*, and *cooled again*, "was an excellent phoſphorus. I muſt "own that, upon the ſtricteſt reſearch into "the work to which he refers, I have not "been able to find any ſuch account. Nor "do I conceive in what manner paper "can be made *red hot*, and afterwards "*cooled*, without being reduced to aſhes. "I ſhould

"I ſhould, nevertheleſs, be greatly obliged "to the learned hiſtorian, who relates the "experiment, for an explanation of his "meaning, if he can point out the paſ"ſage to which his elaborate work re"fers."

Beſides, except that the words *red hot* were not in the treatiſe I was abridging, I have nothing to alter with reſpect to it. For if Mr. Wilſon does not know it at preſent, he may ſatisfy himſelf in half a minute, that white paper always becomes *red* by heat, before it is turned *black*.

Having this occaſion to mention Mr. Wilſon and his book, which I think to be, in ſeveral reſpects, a very valuable one, I muſt farther obſerve, that he takes every opportunity of cavilling at my *Hiſtory*; when, admitting his pretended diſcoveries, which were ſubſequent to the publication of that work, it is not liable to the leaſt juſt exception: ſince, as an *hiſtorian*, I could not but take for granted, that there was no fallacy in the experiments of Mr. Canton and Father Beccaria, eſpecially as they confirm the Newtonian doctrine concerning light.

Father Beccaria had advanced, that the Bolognian phoſphorus emits the very ſame particles of light which it had imbibed; ſo that, if red rays only had been thrown upon it, it would appear red, and if it had been expoſed to blue rays only, it would appear blue, &c. *Phil. Tranſ.* vol. 61. p. 212. Mr. Wilſon endeavoured to repeat theſe experiments, but without ſucceſs; and becauſe, in the manner in which he made them, all his phoſphori appeared of the ſame colour, he concludes, contrary to what Mr. Canton and Father Beccaria had ſuppoſed, that the light which the phoſphorus emits, is not the ſame that it had imbibed; but that there is a tranſmutation of the inflammable principle of the phoſphorus itſelf into light.

I have not endeavoured to aſcertain this fact, not having, as yet, any convenience for experiments of that kind; but I will take the liberty to ſay, that a philoſopher of ſuch a claſs as Father Beccaria, is intitled to the greateſt reſpect; and that his concluſions ſhould not have been controverted, but upon much better grounds than Mr. Wilſon's. For, from the manner in which his experiments were made, I cannot but think them to have been inadequate to the object of them, and that they muſt be conſidered as indeciſive. And whenever the experiments ſhall be made with a ſtronger and purer light, I have very little doubt but that Father Beccaria will appear to have been in this, as well as in all his other numerous experiments, perfectly accurate, and that the concluſion which he draws from them is ſtrictly juſt, though contradicted by Mr. Wilſon. In this, however, I may be miſtaken.

Having proceeded thus far in an account of the miſrepreſentations of my meaning, advancing from a leſs to a greater cauſe of complaint, from ſimple admonition to reprehenſion, I ſhall go one ſtep farther, to take notice of a wilful and moſt wicked perverſion of my meaning, in a buſineſs of much more importance than thoſe which I have mentioned already. If it be ſaid that in this I digreſs too far, let it be conſidered that, in a *preface*, authors have always claimed a right of ſaying whatever they pleaſed concerning themſelves; and not to loſe this right, it muſt now and then be exerciſed. It will be ſeen, however, that, in this digreſſion, I have views not very foreign to the ſubject of a treatiſe addreſſed to philoſophers.

Notwithſtanding my ſtudies and writings are chiefly of a theological nature, and my philoſophical purſuits only occaſional; notwithſtanding, in my *Inſtitutes of natural and revealed religion*, I have an intire volume on the evidences of chriſtianity, in which I flatter myſelf I have placed ſeveral parts of it in a new and ſtronger light, and this from *inclination* only, without a ſhadow of *intereſt* to biaſs me, I have been repreſented in an artful *advertiſement*, frequently repeated in all the Engliſh newſpapers, as not believing in a future ſtate. The author of the advertiſement has, for this baſe purpoſe, quoted the following mutilated ſentence from an *Eſſay* of mine prefixed to my edition of *Dr. Hartley's Obſervations on the human mind*, p. 20.

"I am rather inclined to think, though "the ſubject is beyond our comprehenſion "at preſent, that man doth not conſiſt of "two principles ſo eſſentially different "from one another as *matter* and *ſpirit*, "which are always deſcribed as having "no one common property, by means of "which they can affect or act upon each "other, &c. I rather think that the whole "man is of ſome uniform compoſition, "and that the property of *perception*, as "well as the other powers that are termed "*mental*, is the reſult (whether neceſſary "or not) of ſuch an organical ſtructure "as that of the brain. Conſequently that "the whole man becomes extinct at death, "&c."

The wickedneſs of this repreſentation will appear by reciting the remainder of the ſentence.

——"at death, and that we have no "hope of ſurviving the grave, but what

“ is derived from the ſcheme of revela-
“ tion.”

In the ſame page I alſo obſerve that, though this doctrine favours the opinion of the lower animals differing from us in *degree* only, and not in *kind*, “ it does not “ neceſſarily draw after it the belief of “ their ſurviving death as well as our- “ ſelves; this privilege being derived to “ us by a *poſitive conſtitution*, and depend- “ ing upon the promiſe of God, commu- “ nicated by expreſs revelation to man.”

This affair has been the occaſion of much exultation among *bigots*, as a proof that freedom of thinking in matters of religion leads to infidelity; and *unbelievers*, who have never read any but my philoſophical writings, have conſidered me as one of their fraternity. To the former I ſhall ſay nothing, becauſe it would avail nothing. To the latter, of whom I have more hopes, I would take this opportunity of obſerving (and in this I addreſs myſelf to foreigners more than my own countrymen) that, as they will agree with me in the opinion of the *natural mortality of the ſoul*, which is agreeable to every appearance in nature, it nearly concerns us to conſider whether there be no evidence of a future life of retribution independent of the contrary doctrine, which has no countenance from the ſcriptures *; that it argues extreme narrowneſs of mind, unworthy of the ſpirit of philoſophy, not to extend our views and inquiries beyond the circle of thoſe objects about which natural philoſophy is converſant, which terminate in gaining a knowledge of the viſible ſyſtem of nature; and that it behoves us to conſider whether the great Author of nature has not afforded us ſufficient *data* for knowledge infinitely more intereſting to us, more immediately reſpecting our relation to himſelf, and his gracious proviſion for our improvement and happineſs, not only in this *infancy of our being*, but to a period which has no bounds.

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* In this opinion I am far from being ſingular. It is known to have been the opinion of Luther, and many of the moſt eminent of the firſt reformers. Of late years it has been moſt ably ſupported by the preſent excellent Biſhop of Carliſle, and is now generally adopted by rational chriſtians. The opinion of the *natural immortality of the ſoul*, had its origin in the heathen philoſophy; and having, with other pagan notions, inſinuated itſelf into chriſtianity (which has been miſerably depraved by this means) has been the great ſupport of the popiſh doctrines of *purgatory*, and the *worſhip of the dead*.

Let philoſophers, as certainly becomes their character, conſider *facts*, and the *phenomena of the human mind*, as influenced by facts, and it muſt appear to them to be utterly incredible, that chriſtianity ſhould have ariſen, have been propagated, and have eſtabliſhed itſelf in the world, in the circumſtances in which all hiſtory ſhews that it did ariſe, and was propagated, if it had not been founded on truth and fact; ſuch facts as are ſtrictly the ſubject of hiſtorical inveſtigation.

The common objection againſt religion among philoſophers is, that it was invented by artful intereſted *prieſts*, or wiſe *magiſtrates*: but it is not fact that *chriſtianity* had any ſuch origin. No prieſt was concerned in the invention of it, nor did any civil magiſtrate foſter it: but, on the contrary, it was violently oppoſed by all prieſts, and all magiſtrates, wherever it appeared,

peared, and by its own evidence it triumphed over both. Theſe are *facts* worthy of the attention of philoſophers, as ſuch.

To quit this ſubject for that with which I began this preface, and in which I ſhall be more attended to by philoſophers in general, I would caution my reader not to be too ſanguine in his expectations from the happy train which this branch of philoſophy ſeems to be in. Conſidering the unexampled rapidity with which diſcoveries have hitherto been made in it, the number of perſons in many and diſtant countries now engaged in theſe purſuits, and the emulation that is neceſſarily excited in ſuch circumſtances; and conſidering, at the ſame time, how nearly this ſubject is allied to the moſt general and comprehenſive laws of nature with which we are acquainted; ſome may be apt to imagine, that every year muſt produce diſcoveries equal to all that were made by a Newton or a Boyle; and I am far from ſaying that this may not be the caſe, or that it is very improbable.

But, though I have little doubt, from the train that things are viſibly in, that philoſophical diſcoveries in general will go on with an accelerated progreſs (as indeed they have done ever ſince the revival of letters in Europe) it would be too raſh to infer, from any preſent flattering appearances, that any particular expedition into the undiſcovered regions of ſcience will be crowned with more diſtinguiſhed ſucceſs than another. Nothing is more common, in the hiſtory of all the branches of experimental philoſophy, than the moſt unexpected revolutions of good or bad ſucceſs. In general, indeed, when numbers of ingenious men apply themſelves to one ſubject, that has been *well opened*, the inveſtigation proceeds happily and equably. But, as in the hiſtory of *electricity*, and now in the diſcoveries relating to *air*, light has burſt out from the moſt unexpected quarters, in conſequence of which the greateſt maſters of ſcience have been obliged to recommence their ſtudies, from new and ſimpler elements; ſo it is alſo not uncommon for a branch of ſcience to receive a check, even in the moſt rapid and promiſing ſtate of its growth.

Judging, however, from my preſent views of the ſubject, I am willing to hope that the beginning of this preface will not contribute to raiſe too high expectations. The *incomplete experiments*, indicated in the courſe of both theſe volumes, and eſpecially in the ſecond, will, alone, furniſh matter for, at leaſt, as much experimental inveſtigation as all that I have yet gone through; and I need not tell the real philoſopher, that many of them are of ſuch a nature, as promiſe to reward the ſagacious experimenter with the moſt important diſcoveries, as they evidently border upon, and may lead to, much greater things than any that I have hitherto inveſtigated; and my *hints* for other new experiments, which I have not thought it worth while to trouble the reader with at this time, are more than I have ever had before me ſince I began theſe inquiries. From this I think I may reaſonably infer, that the ſubject is ſo far from being *exhauſted*, that the moſt that can be ſaid of it is, that it is *pretty well opened*, ſo as to exhibit an inviting proſpect to future inveſtigation.

To accelerate this inveſtigation, I have hitherto made the moſt early publication of my obſervations, and have concealed from no perſon whatever any thing that has occurred to me; and though this conduct has expoſed me to ſome inconvenience, I am not yet diſcouraged; but, whoever may avail themſelves of it, I ſhall, for ſome time longer, at leaſt, and I hope through life, perſiſt in the ſame habit of the moſt open and unreſerved communication, private and public.

I have not in this volume, as in the former, a ſection of *conjectures*, *ſpeculations*, and *hints*, becauſe I have not yet ſufficiently reflected upon the *facts* that ſuggeſt them. The facts, however, will furniſh abundant matter to thoſe who are diſpoſed to ſpeculate, and eſpecially on the ſubject of the mutual convertibility, and ultimate identity, of all the acids when combined with ſubſtances in the form of air; but I chuſe to wait for more facts, before I deduce any general theory. In the mean time the field is as open to others as to myſelf.

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SECTION I.

Of VITRIOLIC *Acid Air.*

I Had no ſooner exhibited the *marine acid* in the form of air, than it occurred to me that it might be poſſible to exhibit the other acids alſo in the ſame elegant manner, diveſted of the water with which they had hitherto been combined, and which muſt neceſſarily have been a great obſtruction to the diſcovery of their real natures and affinities; but not being a practical Chemiſt, and living in the country, where I had no acceſs to any perſon of that profeſſion, and indeed not being ſufficiently able to explain my wants, I met with many hindrances in the proſecution of my inquiries into this ſubject.

My firſt ſcheme was to endeavour to get the *vitriolic* acid in the form of air, thinking that it would probably be eaſy to confine it by quickſilver; for as to the nitrous acid, its affinity with quickſilver is ſo great, that I deſpaired of being able to confine it to any purpoſe, as I have obſerved in the former volume, p. 273. I therefore wrote to my friend Mr. Lane, to procure me a quantity of *volatile vitriolic acid*, which is the common vitriolic acid combined with phlogiſton, at the time that I was intent upon the proſecution of my former experiments; but by ſome miſtake of my meaning, a different thing from what I intended was ſent me.

Seeing Mr. Lane the winter following, he told me that if I would only heat any oily or greaſy matter with oil of vitriol, I ſhould certainly make it the very thing I wanted, viz. the *volatile*, or *ſulphureous vitriolic acid*; and accordingly I meant to have proceeded upon this hint, but was prevented from purſuing it, by a variety of engagements, till after the publication of my late treatiſe.

Some time after this, I was in company with Lord Shelburne at the ſeat of Monſ. Trudaine, at Montigny in France, where, with that generous and liberal ſpirit by which that nobleman is diſtinguiſhed, he has a complete apparatus of philoſophical inſtruments, with every other convenience and aſſiſtance for purſuing ſuch philoſophical inquiries as any of his numerous gueſts ſhall chuſe to entertain themſelves with. In this agreeable retreat I met with that eminent philoſopher and chymiſt, Monſ. Montigni, Member of the Royal Academy of Sciences; and converſing with him upon this ſubject, he propoſed our trying to convert oil of vitriol into vapour, by boiling it with a pan of charcoal in a cracked phial. This ſcheme not anſwering our purpoſe, he next propoſed our heating it together with oil of turpentine. Accordingly we went to work upon it, and ſoon produced a quantity of ſome kind of air confined by quickſilver; but our recipient being overturned by the ſuddenneſs of the production of air, we were not able to catch any more than the firſt produce, which was little elſe than the common air which had lodged on the ſurface of the liquor, and which appeared to be a little phlogiſticated, by its not being much affected by a mixture of nitrous air.

Having no opportunity of repeating the experiment at that time, I did nothing with a view to it till my return to England; when, on the 26th of November, 1774, I reſumed the operation, beginning with *olive oil*, and by the help of a more convenient kind of glaſs veſſel, repreſented fig. *a*, which I had procured for theſe and other ſimilar purpoſes, I found very little difficulty in the proſecution of the experiments.

As I wiſh that my reader may enjoy the benefit of my experience, I would caution him, if he chuſe to repeat the experiments, not to put too much oil, or any other ſimilar ſubſtance, to the oil of vitriol, in order to produce this air. I began with uſing about one fifth part of common oil, leaving ſpace enough, as I thought, in the phial, for the ebullition that might be occaſioned by the production of air; but as ſoon as the veſſel was heated to a certain degree, the production of air was exceedingly rapid; and though I withdrew the candle which I had applied to it for that purpoſe, the ebullition continued to increaſe, till, the capacity of the tube not being ſufficient for the tranſmiſſion of the generated air, the cork was driven out of the phial, and all the contents of it exploded.

After this I only ſlightly covered the ſpirit of vitriol in the phial with olive oil, and then the phenomena were ſimilar to thoſe in the former experiment, at the ſame time that the proceſs was more manageable; for, by applying or withdrawing the candle, as I ſaw occaſion, I got what quantity of air I pleaſed; and removing the phial, in this ſtate of ebullition, from one veſſel to another, I filled ſeveral of them with this new ſpecies of air, as eaſily as I had been uſed to do it with the marine acid air; and the whole proceſs was as pleaſing and as elegant. Indeed, this manner of producing air from ſubſtances contained in ſmall phials, and receiving the produce in quickſilver, when it is of ſuch a nature that it cannot be confined by water, has never failed to ſtrike every perſon to whom I have ſhewed it.

The moment that I ſaw the acid of vitriol aſſume the form of air by the addition of phlogiſton, I concluded that the marine acid alſo muſt have been capable of being exhibited in the ſame manner, by means of the phlogiſton which it naturally contains, and which is inſeparable from it; and moreover, that, probably, ſome portion of phlogiſton may be neceſſary to the volatility and elaſticity of all ſubſtances whatever; ſo that the marine acid air may not be preciſely what I had before imagined, viz. the *pure marine acid in the form of air*; but that, though it is by this means exhibited free from water, which, in a variety of reſpects, modifies and reſtrains its action upon various bodies, it is ſtill combined with a portion of phlogiſton. Since, however, all the bodies with which we are acquainted are, in ſome degree, elaſtic, being capable, at leaſt, of being condenſed by cold, and dilated by heat, it may not be poſſible to ſeparate this principle intirely from any ſubſtance in nature; and therefore, in a ſenſe ſufficiently near the truth, it may ſtill be ſaid that the marine acid air is nothing but the marine acid; the phlogiſton it contains being ſo ſmall, as not to be diſcoverable by any of the uſual teſts of its preſence.

Before any air is produced from the mixture of inflammable matter and oil of vitriol, the whole quantity becomes very black; and a quantity of this ſpirit, thus impregnated with phlogiſton, will yield many times more air than an equal quantity of the ſtrongeſt ſpirit of ſalt: but I never meaſured it with any exactneſs.

When the vitriolic acid air is produced in great plenty, the top of the phial in which it is generated is generally filled with white vapours. This air has alſo the ſame appearance as it is tranſmitted through the glaſs tube, and it is ſometimes diſcoverable in the recipient.

Vitriolic acid air is equally tranſparent with marine acid air, and ſeems to have no more affinity with quickſilver; for when confined by quickſilver, the dimenſions of it are not liable to any variation, excepting by heat and cold, juſt like common air; provided there be no moiſture in the recipient, or in the quickſilver. As the reſemblance between theſe two acid airs was ſo great, it was natural for me to have a view to the experiments I had made with the marine acid air, in conducting theſe that relate to the vitriolic acid, which the reader will eaſily perceive.

Water being admitted to the vitriolic acid air abſorbed it about as readily as the marine acid air; and by its union with it muſt have formed the volatile or ſulphureous acid of vitriol. Indeed the reſult of this combination was ſo obvious, that I did not think it neceſſary to make the experiment.

Like the marine acid air, this vitriolic acid air extinguiſhes a candle, but without any peculiar appearance in the colour of the flame, as it goes out, or as it is lighted again, which is obſervable when the experiment is made with the marine acid air. Vitriolic acid air is alſo heavier than common air; for a candle being let down into a veſſel filled with it, was extinguiſhed many times ſucceſſively, and even after it had ſtood a full hour with its mouth expoſed to the common air.

Ice is inſtantly diſſolved in this, as well as in the marine acid air, and the water impregnated with it continues to diſſolve more ice. Upon this occaſion I obſerved that this acid air bears to be expoſed to cold, without any greater diminution of its bulk than common air is ſubject to in the ſame circumſtances; which appears to me to be a ſufficiently proper criterion to diſtinguiſh *air* from *vapour.* In a certain degree of heat, indeed, even *water* may be exhibited in the form of air; but it is a degree of heat that far exceeds what is uſual in our atmoſphere; and in other caſes terms are applied to very great uſe, for the diſtinction of bodies, which, if examined with ſtrictneſs, would be found ultimately to run into one another, the difference between them being in *degree* rather than in *kind:* but a very *great difference* in degree affords a ſufficient foundation for a difference in appellation.

The phenomena which attend the mixing of alkaline air with the marine acid air, were ſo ſtriking, that I had not been many hours in poſſeſſion of the vitriolic acid air without trying whether the effect of the ſame mixture with this acid air would not make a ſimilar appearance, and the experiment fully anſwered my expectations. A like beautiful white cloud was formed the moment that theſe two kinds of air came into contact, the quantity of air was diminiſhed as faſt as the alkaline air was admitted, and the quickſilver roſe almoſt to the top of the receiver.

I obſerved alſo, that when I put the alkaline air to the vitriolic acid air, the white cloud roſe immediately to the top of the veſſel, as in the experiment with the marine acid air; which proves that the alkaline air is, in both caſes, the lighter of the two. In both caſes alſo, if the alkaline air be produced firſt, the acid air being admitted to it, forms a cloud which reſts upon the quickſilver; never extending beyond a very ſmall ſpace, and riſing only as the quickſilver riſes. The ſubſtance that is formed by the union of the alkaline air with the vitriolic acid air, muſt neceſſarily be the *vitriolic ſal ammoniac*; but I made no experiment to aſcertain it. The quantity of this ſalt with which my receivers are coated in theſe experiments is readily diſſolved in water, as in the experiments with the marine acid air. This, however, it will be ſeen, is not the caſe with the ſalt that is formed by another of the acid airs with alkaline air.

The mixture of other kinds of air with vitriolic acid air produced no remarkable appearance whatever. When, however, I had put a quantity of this acid air to a quantity of common air, in order to obſerve whether the former might not part with ſome of its phlogiſton to the latter, though I perceived no immediate diminution of the bulk of air, as in the mixture of nitrous and common air; yet when they had continued together two days, and water being admitted to the mixture had abſorbed the acid air, the common air which remained appeared, by the teſt of nitrous air, to be conſiderably injured; ſo that the vitriolic acid air muſt have communicated ſome of its phlogiſton, which is an effect that is not produced by the marine acid air when mixed with common air. What effect the vitriolic acid air would have had upon other kinds of air, had they continued together a longer time, I cannot tell.

A quantity of this acid air mixed with inflammable air ſtood ſome hours; but when water had been admitted to them, I could not perceive either that the quantity of inflammable air was altered, or that its inflammability was in the leaſt impaired.

I once put equal quantities of marine and vitriolic acid air into the ſame receiver, and obſerved that they mixed without exhibiting any appearance whatever; and when alkaline air was admitted to them, the appearance was the ſame as if it had been admitted to either of them ſingly, the white cloud riſing inſtantly to the top of the veſſel. Had I, after the experiment, examined the *ſalts* which adhered to different parts of the inſide of the veſſel, I might perhaps have diſcovered which of the two acid airs was ſpecifically lighter than the other; but I ſuſpect that they were intimately mixed, and therefore that the ſalt was ſome uniform compoſition, between the common and the vitriolic ſal ammoniac.

I thought it rather extraordinary, that whereas the marine acid, which is reckoned the weakeſt of all the three mineral acids, ſhould, when exhibited in the form of air, be able to diſlodge both the vitriolic and the nitrous acids from ſeveral of their baſes; yet that this vitriolic acid, which is reckoned the ſtrongeſt of the three, when ſeemingly exhibited to equal advantage, by being diveſted of the water with which it is uſually combined, ſhould not, in any inſtance in which I made the experiment, diſlodge either of the other acids from any baſis with which they were united. *Nitre,* *common ſalt*, and *ſal ammoniac*, were all introduced to this air, without either affecting it, or being affected by it.

Vitriolic ether imbibes vitriolic acid air as readily as water imbibes it. The ether, however, was ſoon ſaturated with it, and afterwards was, to all appearance, both as tranſparent, and as inflammable as before.

A piece of *phoſphorus* remained a day and two nights in vitriolic acid air, without ſenſibly affecting it. It gave no light in this air; but the upper ſurface of it turned black, and the ſurface of the quickſilver on which it lay, had a deep yellow or blackiſh kind of ſcum upon it, as if it had been in part diſſolved by the acid.

Iron is readily diſſolved in marine acid air, but is not at all affected by the vitriolic acid air; though, when combined with water, it is ſo powerful a menſtruum for iron. But this, indeed, is the leſs extraordinary, as this acid ceaſes to affect iron when it is ſtrongly concentrated. I kept a number of iron nails in vitriolic acid air two days, without any ſenſible effect either upon the air, or the nails. There was no appearance of their being in the leaſt corroded.

A piece of *liver of ſulphur*, in three days, abſorbed the whole of a quantity of this kind of air, without ſenſibly affecting the colour or appearance of the liver of ſulphur.

Charcoal, which forms inflammable air, by being introduced to marine acid air, only abſorbs the vitriolic acid air; which, however, it does pretty rapidly, and acquires a pungent ſmell from being expoſed to it, without producing any other effect that I could perceive. I made ſeveral pieces of charcoal imbibe as much of this acid air as they could; but, after this, freſh pieces abſorbed the remainder, ſo that the air had only been, as it were, condenſed on its ſurface. This I have obſerved to be the caſe with alkaline air, and in ſome experiments with other kinds of air which cannot be confined but by quickſilver; and I do not clearly underſtand it. The charcoal, in this experiment, was made very dry, or it might have been ſuſpected that the moiſture adhering to it had abſorbed the air.

Vitriolic acid air diſſolved *camphor* pretty readily, and reduced it to a tranſparent liquor. Water being admitted to it, the camphor reaſſumed its natural ſolid form, but ſeemed to have acquired an acidity in its taſte.

I have noticed a very remarkable effect of alkaline air upon alum, rendering it white and opake, as if it had been calcined, but without altering its figure. The ſame, to appearance, is the effect of vitriolic acid air upon *borax*. This ſubſtance abſorbed a pretty large quantity of this air in two days. What remained of the air extinguiſhed a candle. But this effect was probably owing to a ſmall proportion of fixed air that was produced at the ſame time with the vitriolic acid air. I repeated this experiment with borax, and let the proceſs continue three days, when the effect was preciſely the ſame as before, the borax retaining its form, but being rendered white and opake. The acid air had, no doubt, ſeized upon the water which enters into its compoſition, as I conjecture to be the caſe with reſpect to alkaline air and alum.

As it is well known that the common vitriolic acid is changed into volatile or ſulphureous acid of vitriol by fumes of charcoal, if the veſſel in which it is heated has a crack in it, through which the fumes can have acceſs to the acid, I had the curioſity to try whether the ſame effect would not be produced by heating the charçoal in the acid. Accordingly I put ſome bits of *charcoal* into my phial, inſtead of the oil, or other inflammable matter, which I had uſed before; and, applying the flame of a candle, I preſently found that the vitriolic acid air was produced as well as in the former proceſs, and in ſeveral reſpects more conveniently, the production of air being more equable; whereby the diſagreeable effect of a ſudden exploſion is avoided.

It is neceſſary, however, that the charcoal be very well burned, ſo that all air be expelled from it; otherwiſe, there will be a mixture of fixed or inflammable air along with the acid air, eſpecially when a conſiderable degree of heat is applied to produce the air. Having often got vitriolic acid air from charcoal, on account of the eaſy and equable production of it in this manner, I ſeveral times obſerved that there was a conſiderable reſiduum after it had been expoſed to water: and once I found that the reſiduum made lime-water turbid; a ſure ſign of its containing fixed air.

When I endeavoured to procure this air by the ſame proceſs from *ether*, about one half of the produce was permanent and inflammable. The oil of vitriol became perfectly black by this means, as in heating other phlogiſtic matters in it. Afterwards, heating the ſame oil of vitriol and ether, about one fourth of the produce only was inflammable; and had I continued to uſe the ſame mixture, the produce would probably have been leſs and leſs inflammable, and more purely acid, every experiment.

Finding that a great variety of ſubſtances, containing phlogiſton, enabled the oil of vitriol to throw out a permanent acid air, I had ſome ſuſpicion that mere *heat* might do the ſame; but I did not find that there was any foundation for that ſuſpicion. When, indeed, I put nothing into the phial along with the oil of vitriol, but only heated it in a phial cloſed with a common *cork*, air was produced pretty faſt; but the cork was corroded, and the oil of vitriol was as black as if the cork had been dipped into it; ſo that phlogiſton had evidently come from the cork. It was plain, however, that ſome acid vapour had riſen, or the cork could not have been affected by it.

When, however, by the help of Mr. Parker, I got glaſs phials with ground ſtopples, perforated, and drawn out into tubes, ſuch as are repreſented fig. *c*, I found that heating the oil of vitriol in them produced no air whatever; though, for a long time, I gave a ſmall phial as much heat as I poſſibly could, by keeping it ſurrounded with the flames of two large candles. I was not able to make it actually boil with this heat, but a white vapour iſſued from it, and circulated in the top of the phial, riſing in one place, and being condenſed in another.

But though I got no air from the oil of vitriol by this proceſs, air was produced at the ſame time, in a manner that I little expected, and I paid pretty dearly for the diſcovery it occaſioned. Deſpairing to get any air from the longer application of my candles, I withdrew them; but before I could diſengage the phial from the veſſel of quickſilver, a little of it paſſed through the tube into the hot acid; when, inſtantly, it was all filled with denſe white fumes, a prodigious quantity of air was generated, the tube through which it was tranſmitted was broken into many pieces, (I ſuppoſe by the heat that was ſuddenly produced) and part of the hot acid being ſpilled upon my hand, burned it terribly, ſo that the effect of it is viſible to this day. The inſide of the phial was coated with a white ſaline ſubſtance, and the ſmell that iſſued from it was extremely ſuffocating.

This accident taught me what I am ſurprized I ſhould not have ſuſpected before, viz. that ſome *metals* will part with their phlogiſton to hot oil of vitriol, and thereby convert it into a permanent elaſtic air, producing the very ſame effect with oil, charcoal, or any other inflammable ſubſtance.

Not diſcouraged by the diſagreeable accident above-mentioned, the next day I put a little *quickſilver* into the phial with the ground ſtopple and tube, along with the oil of vitriol; when, long before it was boiling hot; air iſſued plentifully from it; and being received in a veſſel of quickſilver, appeared to be genuine vitriolic acid air, exactly like that which I had procured before; being readily imbibed by water, and extinguiſhing a candle in the ſame manner as the other had done. A white ſalt was formed; but what I thought a little remarkable, was, that, whereas in all the former experiments the oil of vitriol turned black before it yielded any air; this was not the caſe here; for it continued colourleſs and tranſparent during the whole proceſs.

After this I repeated the experiment with ſeveral other metals; but with a conſiderable variety in the reſults.

Putting pieces of *iron wire* into the oil of vitriol, a very ſmall quantity of air was produced without heat; but this ſoon ceaſing, I applied the candle, when, with a degree of heat, ſeemingly greater than that at which the air had riſen from the quickſilver in the ſame circumſtances, air was produced in great plenty. When I had got about three ounce meaſures of it, I admitted water to it, and about four fifths of the whole was preſently abſorbed. The remainder was inflammable, burning very red.

Had the oil of vitriol been more concentrated, or had I continued the proceſs longer, a greater proportion of the air would probably have been acid, and leſs of it inflammable. In this experiment the oil of vitriol became very opaque, being of a deep grey colour. The iron which had undergone this proceſs, and which I had laid aſide without any expectation, was, in a few days, covered with a whitiſh duſt; and after it had been wiped clean, was covered again with the ſame matter. It is very much unlike the ruſting of iron in other circumſtances.

About one third of the produce of air from *zinc*, was acid, and the remainder inflammable. Indeed it was evident that the acid had a conſiderable effect upon the zinc before the application of the candle, ſmall bubbles of air continually riſing from it. The oil of vitriol, which had been uſed in this proceſs, after a long time, depoſited a white matter, which I ſuppoſe to be the *flowers of zinc.*

Copper, treated in the ſame manner, yielded air very freely, with about the ſame degree of heat that quickſilver had required, and the air continued to be generated with very little application of more heat. The whole produce was vitriolic acid air, and no part of it inflammable. The oil of vitriol remained a long time turbid, but at length depoſited a browniſh matter.

The ſolution of *ſilver* in the ſame manner, had the very ſame reſult, all the air being acid, and no part of it inflammable. The oil of vitriol acquired a kind of orange-colour, and depoſited nothing.

With a very great degree of heat *lead* yielded a little air, which was wholly acid, and had nothing inflammable in it.

Gold yielded no air at all in this treatment; but the oil of vitriol acquired the ſame orange-colour that it had when the ſilver had been heated in it.

Neither had this treatment of *platina* any ſenſible effect. What I made uſe of was ſome which I had been favoured with from Dr. Irving carefully purged from iron.

In moſt of theſe proceſſes, air ſeems to iſſue from the ſubſtances immediately upon the application of heat, and ſometimes without it: and this firſt produce of air forms bubbles, which continue ſome time on the ſurface of the liquor. But it ſeems to be nothing more than the common air which had adhered to the ſurfaces of thoſe ſubſtances, or had been confined in the little cavities near the ſurface, when they happened to be rough. For this ſeeming production of air ſoon ceaſes, and no more is produced without a much greater degree of heat; and when the genuine acid air begins to riſe, bubbles formed by it break inſtantly, like bubbles of air in ſpirit of wine, and there is nothing like froth on the ſurface of the oil of vitriol.

As ſulphur is formed by the union of phlogiſton with oil of vitriol highly concentrated and very hot, I imagined that by heating ſubſtances containing phlogiſton in vitriolic acid air, I could not fail to produce ſulphur; but I tried charcoal in this manner without the effect that I had expected from it. The heat of a burning lens thrown upon it in this acid air, only made it throw out that quantity of the air, which, as I have obſerved before, is condenſed upon its ſurface, or imbibed by it. The air that was unabſorbed after this operation was in part fixed, and in part inflammable, having come from the charcoal.

There was frequently, however, the appearance of ſulphur produced upon the mixture of alkaline air with vitriolic acid air; for the inſide of the tube would be covered with a perfectly yellow matter. But this colour goes off in time, and nothing but a white ſaline ſubſtance remains. This yellow appearance I firſt obſerved when I had produced the vitriolic acid air from ether; but afterwards I found the ſame effect when it was produced from charcoal, and ſtill more remarkably when it had been produced from copper. Why this yellow colour ſhould not be permanent, I do not underſtand.

SECTION II.

Of VEGETABLE *Acid Air.*

Having hit upon a method of exhibiting ſome of the acids in the form of air, nothing could be eaſier than to extend this proceſs to the reſt. I had nothing to do but either to procure the acid in a liquid form, viz. combined with water, and then expel the air by heat; or to find ſome ſolid ſubſtance in which it was combined, and, diſlodging it by ſome ſtronger acid, to receive the generated air in quickſilver.

To procure the *vegetable acid air*, I was favoured by Dr. Higgins, with a quantity of exceedingly ſtrong concentrated acid of vinegar, from which, by means of heat, and with the apparatus repreſented vol. I, plate 2. fig. 8. I could eaſily expel as much air, as from an equal quantity of ſpirit of ſalt. I found, however, that unleſs the apparatus was furniſhed with a ſmall recipient, to intercept the liquor that might be thrown out of the veſſel by boiling, I could not (except at the very firſt) procure this acid air free from moiſture:

but with this proviſion I eaſily got the air as perfectly dry as I could wiſh.

This vegetable acid air extinguiſhes the flame of a candle, exactly like the vitriolic acid air, viz. without any particular colour of the flame in going out, or in lighting again.

Upon putting alkaline air to vegetable acid air, the white cloud obſervable in ſimilar mixtures was inſtantly formed, and roſe at once to the top of the veſſel, as in the caſe of the other acid airs. The ſides of the veſſel in which this mixture was made, were tinged with yellow, as in the ſame proceſs with vitriolic acid air; which to me is a puzzling fact, as I do not know that ſuch a ſulphur (if the ſubſtance be ſulphur) was ever known to be formed without the vitriolic acid. At firſt I imagined that this colour had come from ſomething contained in the ingredients for making the alkaline air; but I preſently found, that when I put the alkaline air, from the very ſame preparation, to marine acid air, the ſalt formed by them was perfectly white, without the leaſt tinge of yellow.

The affinities both of the marine and of the vitriolic acids in the form of air, have been ſeen to be conſiderably different from what they are when combined, as uſual, with water; but in all the trials that I have made, the vegetable acid, even in this moſt advantageous form of air, appears to be weaker than any of the three mineral acids, exactly as might be concluded from what was known before concerning it. For this vegetable acid air was not able to decompoſe any ſubſtance into the compoſition of which any of the mineral acids entered. It made no impreſſion upon *brimſtone*, *ſalt-petre*, *common ſalt*, or *ſal ammoniac*; nor yet upon *borax*.

Charcoal imbibes vegetable acid air very faſt, and afterwards the ſmell of it is extremely pungent; but the air which remains ſeems not to have been altered by any thing that it had got from the charcoal.

Liver of ſulphur imbibes vegetable acid air but ſlowly, and is neither diſcoloured nor diſſolved by it. When only one tenth part of the air remained, I examined it, and found it to have nothing inflammable in it, which was the only effect that I had expected from it.

Water imbibes vegetable acid air as readily as any of the other acid airs. I once endeavoured to aſcertain the quantity of this air that a given a given quantity of water would imbibe; and to meaſure the increaſe of weight and bulk that it might acquire by this impregnation, as I had in ſome meaſure done with reſpect to the marine acid, and alkaline airs: but the experiment did not ſucceed to my wiſh; and I did not think it worth my while to attempt it again.

For this purpoſe I put a ſmall quantity of water into a glaſs tube; but it was no ſooner introduced to the acid air, through the quickſilver, by which it was confined, than a ſmall bubble of common air at the cloſed end of the tube began to ſwell, and it continued to do ſo till it threw out all the water. The caſe was the ſame when the end of the tube was hermetically ſealed. I had the ſame reſult from *ſpirit of wine* introduced into this acid air, in the ſame circumſtances, only the effect was produced much quicker. With *oil of turpentine* this effect was produced more quickly ſtill; but with *olive oil* much more ſlowly.

From this experiment I was led to imagine, that common air received a great expanſion by the effluvium of this vegetable acid; and I therefore expected that a quantity of the liquid acid admitted to common air, confined by quickſilver, would make it expand as ether had done; but this was ſo far from being the caſe, that, after ſome time, the air appeared to be diminiſhed, and extinguiſhed a candle, ſo that it muſt have got phlogiſton from the acid.

I made a ſecond experiment of this kind, the reſult of which was, that a quantity of common air, which had been expoſed ſix weeks to the effluvium of a ſmall quantity of the liquid vegetable acid (contained in a cup, which ſwam upon the ſurface of the water, by which the air was confined) was ſo far injured, that two meaſures of it, and one of nitrous air did not occupy the ſpace of quite two meaſures; that is, it was almoſt as noxious as it could be made by any phlogiſtic proceſs.

Suſpecting that the water, which was rather foul, might have contributed to this injury, I expoſed, for the laſt five weeks of the time, an equal quantity of common air, in a jar of the ſame ſize, ſtanding in the ſame trough of water, and in all other reſpects in ſimilar circumſtances. But this air, though a little injured, was hardly to be diſtinguiſhed from common air; ſo that there could be no doubt but that, in the laſt-mentioned experiment, the injury which the air had received, came from the effluvia of the vegetable acid.

Vegetable acid air is abſorbed pretty readily by *olive oil.* A quantity of this oil abſorbed about ten times its bulk of this air; and from being of a yellowiſh colour, as this oil naturally is, it became almoſt colourleſs, like water; which I thought not a little remarkable; as all the other acid airs deepen the colour of every ſpecies of oil, making them brown, and at the ſame time viſcid, approaching to the conſiſtence of reſin; whereas this oil, in the experiment juſt now mentioned, became rather leſs viſcid than before, a little approaching to the limpidity of water, or rather, more reſembling an eſſential oil.

SECTION III.

Of DEPHLOGISTICATED *Air, and of the conſtitution of the Atmoſphere,*

The contents of this ſection will furniſh a very ſtriking illuſtration of the truth of a remark, which I have more than once made in my philoſophical writings, and which can hardly be too often repeated, as it tends greatly to encourage philoſophical inveſtigations; viz. that more is owing to what we call *chance*, that is, philoſophically ſpeaking, to the obſervation of *events ariſing from unknown cauſes*, than to any proper *deſign*, or pre-conceived *theory* in this buſineſs. This does not appear in the works of thoſe who write *ſynthetically* upon theſe ſubjects; but would, I doubt not, appear very ſtrikingly in thoſe who are the moſt celebrated for their philoſophical acumen, did they write *analytically* and ingenuouſly.

For my own part, I will frankly acknowledge, that, at the commencement of the experiments recited in this ſection, I was ſo far from having formed any hypotheſis that led to the diſcoveries I made in purſuing them, that they would have appeared very improbable to me had I been told of them; and when the deciſive facts did at length obtrude themſelves upon my notice, it was very ſlowly, and with great heſitation, that I yielded to the evidence of my ſenſes. And yet, when I re-conſider the matter, and compare my laſt diſcoveries relating to the conſtitution of the atmoſphere with the firſt, I ſee the cloſeſt and the eaſieſt connexion in the world between them, ſo as to wonder that I ſhould not have been led immediately from the one to the other. That this was not the caſe, I attribute to the force of prejudice, which, unknown to ourſelves, biaſſes not only our *judgments*, properly ſo called, but even the perceptions of our ſenſes: for we may take a maxim ſo ſtrongly for granted, that the plaineſt evidence of ſenſe will not intirely change, and often hardly modify our perſuaſions; and the more ingenious a man is, the more effectually he is entangled in his errors; his ingenuity only helping him to deceive himſelf, by evading the force of truth.

There are, I believe, very few maxims in philoſophy that have laid firmer hold upon the mind, than that air, meaning atmoſpherical air (free from various foreign matters, which were always ſuppoſed to be diſſolved, and intermixed with it) is *a ſimple elementary ſubſtance*, indeſtructible, and unalterable, at leaſt as much ſo as water is ſuppoſed to be. In the courſe of my inquiries, I was, however, ſoon ſatisfied that atmoſpherical air is not an unalterable thing; for that the phlogiſton with which it becomes loaded from bodies burning in it, and animals breathing it, and various other chemical proceſſes, ſo far alters and depraves it, as to render it altogether unfit for inflammation, reſpiration, and other purpoſes to which it is ſubſervient; and I had diſcovered that agitation in water, the proceſs of vegetation, and probably other natural proceſſes, by taking out the ſuperfluous phlogiſton, reſtore it to its original purity. But I own I had no idea of the poſſibility of going any farther in this way, and thereby procuring air purer than the beſt common air. I might, indeed, have naturally imagined that ſuch would be air that ſhould contain leſs phlogiſton than the air of the atmoſphere; but I had no idea that ſuch a compoſition was poſſible.

It will be ſeen in my laſt publication, that, from the experiments which I made on the marine acid air, I was led to conclude, that common air conſiſted of ſome acid (and I naturally inclined to the acid that I was then operating upon) and phlogiſton; becauſe the union of this acid vapour and phlogiſton made inflammable air; and inflammable air, by agitation in water, ceaſes to be inflammable, and becomes reſpirable. And though I could never make it quite ſo good as common air, I thought it very probable that vegetation, in more favourable circumſtances than any in which I could apply it, or ſome other natural proceſs, might render it more pure.

Upon this, which no perſon can ſay was an improbable ſuppoſition, was founded my conjecture, of volcanos having given birth to the atmoſphere of this planet, ſupplying it with a permanent air, firſt inflammable, then deprived of its inflammability by agitation in water, and farther purified by vegetation.

Several of the known phenomena of the *nitrous acid* might have led me to think, that this was more proper for the conſtitution of the atmoſphere than the marine acid: but my thoughts had got into a different train, and nothing but a ſeries of obſervations, which I ſhall now diſtinctly relate, compelled me to adopt another hypotheſis, and brought me, in a way of which I had then no idea, to the ſolution of the great problem, which my reader will perceive I have had in view ever ſince my diſcovery that the atmoſpherical air is alterable, and therefore that it is not an elementary ſubſtance, but a *compoſition* viz. what this compoſition is, or *what is the thing that we breathe*, and how is it to be made from its conſtituent principles.

At the time of my former publication, I was not poſſeſſed of a *burning lens* of any conſiderable force; and for want of one, I could not poſſibly make many of the experiments that I had projected, and which, in theory, appeared very promiſing. I had, indeed, a *mirror* of force ſufficient for my purpoſe. But the nature of this inſtrument is ſuch, that it cannot be applied, with effect, except upon ſubſtances that are capable of being ſuſpended, or reſting on a very ſlender ſupport. It cannot be directed at all upon any ſubſtance in the form of *powder*, nor hardly upon any thing that requires to be put into a veſſel of quickſilver; which appears to me to be the moſt accurate method of extracting air from a great variety of ſubſtances, as was explained in the Introduction to this volume. But having afterwards procured a lens of twelve inches diameter, and twenty inches focal diſtance, I proceeded with great alacrity to examine, by the help of it, what kind of air a great variety of ſubſtances, natural and factitious, would yield, putting them into the veſſels repreſented fig. *a*, which I filled with quickſilver,

ſilver, and kept inverted in a baſon of the ſame. Mr. Warltire, a good chymiſt, and lecturer in natural philoſophy, happening to be at that time in Calne, I explained my views to him, and was furniſhed by him with many ſubſtances, which I could not otherwiſe have procured.

With this apparatus, after a variety of other experiments, an account of which will be found in its proper place, on the 1ſt of Auguſt, 1774, I endeavoured to extract air from *mercurius calcinatus per ſe*; and I preſently found that, by means of this lens, air was expelled from it very readily. Having got about three or four times as much as the bulk of my materials, I admitted water to it, and found that it was not imbibed by it. But what ſurprized me more than I can well expreſs, was, that a candle burned in this air with a remarkably vigorous flame, very much like that enlarged flame with which a candle burns in nitrous air, expoſed to iron or liver of ſulphur; but as I had got nothing like this remarkable appearance from any kind of air beſides this particular modification of nitrous air, and I knew no nitrous acid was uſed in the preparation of *mercurius calcinatus*, I was utterly at a loſs how to account for it.

In this caſe, alſo, though I did not give ſufficient attention to the circumſtance at that time, the flame of the candle, beſides being larger, burned with more ſplendor and heat than in that ſpecies of nitrous air; and a piece of red-hot wood ſparkled in it, exactly like paper dipped in a ſolution of nitre, and it conſumed very faſt; an experiment which I had never thought of trying with nitrous air.

At the ſame time that I made the above-mentioned experiment, I extracted a quantity of air, with the very ſame property, from the common *red precipitate*, which being produced by a ſolution of mercury in ſpirit of nitre, made me conclude that this peculiar property, being ſimilar to that of the modification of nitrous air above mentioned, depended upon ſomething being communicated to it by the nitrous acid; and ſince the *mercurius calcinatus* is produced by expoſing mercury to a certain degree of heat, where common air has acceſs to it, I likewiſe concluded that this ſubſtance had collected ſomething of *nitre*, in that ſtate of heat, from the atmoſphere.

This, however, appearing to me much more extraordinary than it ought to have done, I entertained ſome ſuſpicion that the mecurius calcinatus, on which I had made my experiments, being bought at a common apothecary's, might, in fact, be nothing more than red precipitate; though, had I been any thing of a practical chymiſt, I could not have entertained any ſuch ſuſpicion. However, mentioning this ſuſpicion to Mr. Warltire, he furniſhed me with ſome that he had kept for a ſpecimen of the preparation, and which, he told me, he could warrant to be genuine. This being treated in the ſame manner as the former, only by a longer continuance of heat, I extracted much more air from it than from the other.

This experiment might have ſatisfied any moderate ſceptic: but, however, being at Paris in the October following, and knowing that there were ſeveral very eminent chymiſts in that place, I did not omit the opportunity, by means of my friend Mr. Magellan, to get an ounce of mercurius calcinatus prepared by Mr. Cadet, of the genuineneſs of which there could not poſſibly be any ſuſpicion; and at the ſame time, I frequently mentioned my ſurprize at the kind of air which I had got from this preparation to Mr Lavoiſier, Mr. le Roy, and ſeveral other philoſophers, who honoured me with their notice in that city; and who, I dare ſay, cannot fail to recollect the circumſtance.

At the ſame time, I had no ſuſpicion that the air which I had got from the mercurius calcinatus was even wholeſome, ſo far was I from knowing what it was that I had really found; taking it for granted, that it was nothing more than ſuch kind of air as I had brought nitrous air to be by the proceſſes above mentioned; and in this air I have obſerved that a candle would burn ſometimes quite naturally, and ſometimes with a beautiful enlarged flame, and yet remain perfectly noxious.

At the ſame time that I had got the air above mentioned from mercurius calcinatus and the red precipitate, I had got the ſame kind from *red lead* or *minium.* In this proceſs, that part of the minium on which the focus of the lens had fallen, turned yellow. One third of the air, in this experiment, was readily abſorbed by water, but, in the remainder, a candle burned very ſtrongly, and with a crackling noiſe.

That fixed air is contained in red lead I had obſerved before; for I had expelled it by the heat of a candle, and had found it to be very pure. See Vol. I. p. 192. I imagine it requires more heat than I then uſed to expel any of the other kind of air.

This experiment with *red lead* confirmed me more in my ſuſpicion, that the *mercurius calcinatus* muſt get the property of yielding this kind of air from the atmoſphere, the proceſs by which that preparation, and this of red lead is made, being ſimilar. As I never make the leaſt ſecret of any thing that I obſerve, I mentioned this experiment alſo, as well as thoſe with the mercurius calcinatus, and the red precipitate, to all my philoſophical acquaintance at Paris, and elſewhere; having no idea, at that time, to what theſe remarkable facts would lead.

Preſently after my return from abroad, I went to work upon the *mercurius calcinatus*, which I had procured from Mr. Cadet; and, with a very moderate degree of heat, I got from about one fourth of an ounce of it, an ounce-meaſure of air, which I obſerved to be not readily imbibed, either by the ſubſtance itſelf from which it had been expelled (for I ſuffered them to continue a long time together before I transferred the air to any other place) or by water, in which I ſuffered this air to ſtand a conſiderable time before I made any experiment upon it.

In this air, as I had expected, a candle burned with a vivid flame; but what I obſerved new at this time, (Nov. 19,) and which ſurprized me no leſs than the fact I had diſcovered before, was, that, whereas a few moments agitation in water will deprive the modified nitrous air of its property of admitting a candle to burn in it; yet, after more than ten times as much agitation as would be ſufficient to produce this alteration in the nitrous air, no ſenſible change was produced in this. A candle ſtill burned in it with a ſtrong flame; and it did not, in the leaſt, diminiſh common air, which I have obſerved that nitrous air, in this ſtate, in ſome meaſure, does.

But I was much more ſurprized, when, after two days, in which this air had continued in contact with water (by which it was diminiſhed about one twentieth of its bulk) I agitated it violently in water about five minutes, and found that a candle ſtill burned in it as well as in common air. The ſame degree of agitation would have made phlogiſticated nitrous air fit for reſpiration indeed, but it would certainly have extinguiſhed a candle.

Theſe facts fully convinced me, that there muſt be a very material difference between the conſtitution of the air from mercurius calcinatus, and that of phlogiſticated nitrous air, notwithanding their reſemblance in ſome particulars. But though I did not doubt that the air from *mercurius calcinatus* was fit for reſpiration, after being agitated in water, as every kind of air without exception, on which I had tried the experiment, had been, I ſtill did not ſuſpect that it was reſpirable in the firſt inſtance; ſo far was I from having any idea of this air being, what it really was, much ſuperior, in this reſpect, to the air of the atmoſphere.

In this ignorance of the real nature of this kind of air, I continued from thistime (November) to the 1ſt of March following; having, in the mean time, been intent upon my experiments on the vitriolic acid air above recited, and the various modifications of air produced by ſpirit of nitre, an account of which will follow. But in the courſe of this month, I not only aſcertained the nature of this kind of air, though very gradually, but was led by it to the complete diſcovery of the conſtitution of the air we breathe.

Till this 1ſt of March, 1775, I had ſo little ſuſpicion of the air from mercurius calcinatus, &c. being wholeſome, that I had not even thought of applying to it the teſt of nitrous air; but thinking (as my reader muſt imagine I frequently muſt have done) on the candle burning in it after long agitation in water, it occurred to me at laſt to make the experiment; and putting one meaſure of nitrous air to two meaſures of this air, I found, not only that it was diminiſhed, but that it was diminiſhed quite as much as common air, and that the redreſs of the mixture was likewiſe equal to that of a ſimilar mixture of nitrous and common air.

After this I had no doubt but that the air from mercurius calcinatus was fit for reſpiration, and that it had all the other properties of genuine common air. But I did not take notice of what I might have obſerved, if I had not been ſo fully poſſeſſed by the notion of there being no air better than common air, that the redneſs was really deeper, and the diminution ſomething greater than common air would have admitted.

Moreover, this advance in the way of truth, in reality, threw me back into error, making me give up the hypotheſis I had firſt formed, viz. that the mercurius calcinatus had extracted ſpirit of nitre from the air; for I now concluded, that all the conſtituent parts of the air were equally, and in their proper proportion, imbibed in the preparation of this ſubſtance, and alſo in the proceſs of making red lead. For at the ſame time that I made the above-mentioned experiment on the air from mercurius calcinatus, I likewiſe obſerved that the air which I had extracted from red lead, after the fixed air was waſhed out of it, was of the ſame nature, being diminiſhed by nitrous air like common air: but, at the ſame time, I was puzzled to find that air from the red precipitate was diminiſhed in the ſame manner, though the proceſs for making this ſubſtance is quite different from that of making the two others. But to this circumſtance I happened not to give much attention.

I wiſh my reader be not quite tired with the frequent repetition of the word *ſurprize*, and others of ſimilar import; but I muſt go on in that ſtyle a little longer. For the next day I was more ſurprized than ever I had been before, with finding that, after the above-mentioned mixture of nitrous air and the air from mercurius calcinatus, had ſtood all night, (in which time the whole diminution muſt have taken place; and, conſequently, had it been common air, it muſt have been made perfectly noxious, and intirely unfit for reſpiration or inflammation) a candle burned in it, and even better than in common air.

I cannot, at this diſtance of time, recollect what it was that I had in view in making this experiment; but I know I had no expectation of the real iſſue of it. Having acquired a conſiderable degree of readineſs in making experiments of this kind, a very ſlight and evaneſcent motive would be ſufficient to induce me to do it. If, however, I had not happened, for ſome other purpoſe, to have had a lighted candle before me, I ſhould probably never have made the trial; and the whole train of my future experiments relating to this kind of air might have been prevented.

Still, however, having no conception of the real cauſe of this phenomenon, I conſidered it as ſomething very extraordinary; but as a property that was peculiar to air extracted from theſe ſubſtancees, and *adventitious*; and I always ſpoke of the air to my acquaintance as being ſubſtantially the ſame thing with common air. I particularly remember my telling Dr. Price, that I was myſelf perfectly ſatisfied of its being common air, as it appeared to be ſo by the teſt of nitrous air; though, for the ſatisfaction of others, I wanted a mouſe to make the proof quite complete.

On the 8th of this month I procured a mouſe, and put it into a glaſs veſſel, containing

ing two ounce-meaſures of the air from mercurius calcinatus. Had it been common air, a full-grown mouſe, as this was, would have lived in it about a quarter of an hour. In this air, however, my mouſe lived a full half hour; and though it was taken out ſeemingly dead, it appeared to have been only exceedly chilled; for, upon being held to the fire, it preſently revived, and appeared not to have received any harm from the experiment.

By this I was confirmed in my concluſion, that the air extracted from mercurius calcinatus, &c. was, *at leaſt*, *as good* as common air; but I did not certainly conclude that it was any *better*; becauſe, though one mouſe would live only a quarter of an hour in a given quantity of air, I knew it was not impoſſible but that another mouſe might have lived in it half an hour; ſo little accuracy is there in this method of aſcertaining the goodneſs of air: and indeed I have never had recourſe to it for my own ſatisfaction, ſince the diſcovery of that moſt ready, accurate, and elegant teſt that nitrous air furniſhes. But in this caſe I had a view to publiſhing the moſt generally-ſatisfactory account of my experiments that the nature of the thing would admit of.

This experiment with the mouſe, when I had reflected upon it ſome time, gave me ſo much ſuſpicion that the air into which I had put it was better than common air, that I was induced, the day after, to apply the teſt of nitrous air to a ſmall part of that very quantity of air which the mouſe had breathed ſo long; ſo that, had it been common air, I was ſatisfied it muſt have been very nearly, if not altogether, as noxious as poſſible, ſo as not to be affected by nitrous air; when, to my ſurprize again, I found that though it had been breathed ſo long, it was ſtill better than common air. For after mixing it with nitrous air, in the uſual proportion of two to one, it was diminiſhed in the proportion of $4\frac{1}{2}$ to $3\frac{1}{2}$; that is, the nitrous air had made it two ninths leſs than before, and this in a very ſhort ſpace of time; whereas I had never found that, in the longeſt time, any common air was reduced more than one fifth of its bulk by any proportion of nitrous air, nor more than one fourth by any phlogiſtic proceſs whatever. Thinking of this extraordinary fact upon my pillow, the next morning I put another meaſure of nitrous air to the ſame mixture, and, to my utter aſtoniſhment, found that it was farther diminiſhed to almoſt one half of its original quantity. I then put a third meaſure to it; but this did not diminiſh it any farther: but,

however,

however, left it one meaſure leſs than it was even after the mouſe had been taken out of it.

Being now fully ſatisfied that this air, even after the mouſe had breathed it half an hour, was much better than common air; and having a quantity of it ſtill left, ſufficient for the experiment, viz. an ounce-meaſure and a half, I put the mouſe into it; when I obſerved that it ſeemed to feel no ſhock upon being put into it, evident ſigns of which would have been viſible, if the air had not been very wholeſome; but that it remained perfectly at its eaſe another full half hour, when I took it out quite lively and vigorous. Meaſuring the air the next day, I found it to be reduced from $1\frac{1}{2}$ to $\frac{2}{3}$ of an ounce-meaſure. And after this, if I remember well (for in my *regiſter* of the day I only find it noted, that it was *conſiderably diminiſhed* by nitrous air) it was nearly as good as common air. It was evident, indeed, from the mouſe having been taken out quite vigorous, that the air could not have been rendered very noxious.

For my farther ſatisfaction I procured another mouſe, and putting it into leſs than two ounce-meaſures of air extracted from mercurius calcinatus and air from red precipitate (which, having found them to be of the ſame quality, I had mixed together) it lived three quarters of an hour. But not having had the precaution to ſet the veſſel in a warm place, I ſuſpect that the mouſe died of cold. However, as it had lived three times as long as it could probably have lived in the ſame quantity of common air, and I did not expect much accuracy from this kind of teſt, I did not think it neceſſary to make any more experiments with mice.

Being now fully ſatisfied of the ſuperior goodneſs of this kind of air, I proceeded to meaſure that degree of purity, with as much accuracy as I could, by the teſt of nitrous air; and I began with putting one meaſure of nitrous air to two meaſures of this air, as if I had been examining common air; and now I obſerved that the diminution was evidently greater than common air would have ſuffered by the ſame treatment. A ſecond meaſure of nitrous air reduced it to two thirds of its original quantity, and a third meaſure to one half. Suſpecting that the diminution could not proceed much farther, I then added only half a meaſure of nitrous air, by which it was diminiſhed ſtill more; but not much, and another half meaſure made it more than half of its original quantity; ſo that, in this caſe, two meaſures of this air took more than two meaſures of nitrous air, and yet remained leſs than half of what it was. Five meaſures brought it pretty exactly to its original dimenſions.

At the ſame time, air from the *red precipitate* was diminiſhed in the ſame proportion as that from *mercurius calcinatus*, five meaſures of nitrous air being received by two meaſures of this without any increaſe of dimenſions. Now as common air takes about one half of its bulk of nitrous air, before it begins to receive any addition to its dimenſions from more nitrous air, and this air took more than four half-meaſures before it ceaſed to be diminiſhed by more nitrous air, and even five half-meaſures made no addition to its original dimenſions, I conclude that it was between four and five times as good as common air. It will be ſeen that I have ſince procured air better than this, even between five and ſix times as good as the beſt common air that I have ever met with.

Being now fully ſatisfied with reſpect to the *nature* of this new ſpecies of air, viz. that, being capable of taking more phlogiſton from nitrous air, it therefore originally contains leſs of this principle; my next inquiry was, by what means it comes to be ſo pure, or philoſophically ſpeaking, to be ſo much *dephlogiſticated*; and ſince the red lead yields the ſame kind of air with mercurius calcinatus, though mixed with common air, and is a much cheaper material, I proceeded to examine all the preparations of lead, made by heat in the open air, to ſee what kind of air they would yield, beginning with the *grey calx*, and ending with *litharge*.

The red lead which I uſed for this purpoſe yielded a conſiderable quantity of dephlogiſticated air, and very little fixed air; but to what circumſtance in the preparation of this lead, or in the keeping of it, this difference is owing, I cannot tell. I have frequently found a very remarkable difference between different ſpecimens of red lead in this reſpect, as well as in the purity of the air which they contain. This difference, however, may ariſe in a great meaſure, from the care that is taken to extract the fixed air from it. In this experiment two meaſures of nitrous air being put to one meaſure of this air, reduced it to one third of what it was at firſt, and nearly three times its bulk of nitrous air made very little addition to its original dimenſions; ſo that this air was exceedingly pure, and better than any that I had procured before.

The preparation called *maſſicot* (which is ſaid to be a ſtate between the grey calx and the red lead) alſo yielded a conſiderable quantity of air, of which about one half was fixed air, and the remainder was ſuch, that when an equal quantity of nitrous air was put to it, it was ſomething leſs than at firſt; ſo that this air was about twice as pure as common air.

I thought it ſomething remarkable, that in the preparations of lead by heat, thoſe before and after theſe two, viz. the red lead and *maſſicot*, yielded only fixed air. I would alſo obſerve, by the way, that a very ſmall quantity of air was extracted from *lead ore* by the burning lens. The bulk of it was eaſily abſorbed by water. The remainder was not affected by nitrous air, and it extinguiſhed a candle.

I got a very little air by the ſame proceſs from the *grey calx of lead*, of preciſely the ſame quality with the former. That part of it which was not affected by nitrous air extinguiſhed a candle, ſo that both of them may be ſaid to have yielded fixed air, only with a larger portion than uſual, of that part of it which does not unite with water.

Litharge (which is a ſtate that ſucceeds the red lead) yielded air pretty readily; but this alſo was fixed air. That which was not abſorbed by water, was not affected by nitrous air.

Much more than I had any opportunity of doing remains to be done, in order to aſcertain upon what circumſtances, in theſe preparations of lead, the quality of the air which they contain, depends. It can only be done by ſome perſon who ſhall carefully attend to the proceſſes, ſo as to ſee himſelf in what manner they are made, and examine them in all their different ſtates. I very much wiſhed to have attempted ſomething of this kind myſelf, but I found it impoſſible in my ſituation. However, I got Dr. Higgins (who furniſhed me with ſeveral preparations that I could not eaſily have procured elſewhere) to make me a quantity of red lead, that I might, at leaſt, try it when *freſh made*, and after keeping it ſome time in different circumſtances; and though, by the help of this preparation, I did not do the thing that I expected, I did ſomething elſe, much more conſiderable.

This freſh made red lead had a yellowiſh caſt, and had in it ſeveral pieces intirely yellow. I tried it immediately, in the ſame manner in which I had made the preceding experiments, viz. with the burning lens in quickſilver, and found that it yielded very little air, and with great difficulty; requiring the application of a very intenſe heat. With an equal quantity of nitrous air, a part of this air was reduced to one half of its original bulk, and 3½ meaſures ſaturated it. The air, therefore, was very pure, and the quantity that it yielded being very ſmall, it proved to be in a very favourable ſtate for aſcertaining on what circumſtances its acquiring this air depended.

My object now was to bring this freſh made red lead, which yielded very little air, to that ſtate in which other red lead had yielded a conſiderable quantity; and taking it, in a manner, for granted, in conſequence of the reaſoning intimated above, that red lead muſt imbibe from the atmoſphere ſome kind of acid, in order to acquire that property, I took three ſeparate half-ounces of this freſh made red lead, and moiſtened them till they made a kind of paſte, with each of the three mineral acids, viz. the vitriolic, the marine, and the nitrous; and as I intended to make the experiments in a gun-barrel, leſt the iron ſhould be too much affected by them, I dried all theſe mixtures, till they were perfectly hard; then pulverizing them, I put them ſeparately into my gun-barrel, filled up to the mouth with pounded flint, which I had found by trial to yield little, or no air when treated in this manner. I had alſo found that no quantity of air, ſufficient to make an experiment, could be procured from an equal quantity of this red lead by this proceſs.

Thoſe portions of the red lead which had been moiſtened with the vitriolic and marine acids became white; but that which had been moiſtened with the nitrous acid, had acquired a deep brown colour. The mixtures with the nitrous and marine acids dried pretty readily, but that with the vitriolic acid was never perfectly dry; but a great part of it remained in the form of a ſoftiſh paſte.

Neither the vitriolic nor the marine acid mixtures gave the leaſt air when treated in the manner above mentioned; but the moment that the compoſition into which the *nitrous* acid had entered became warm, air began to be produced; and I received the produce in quickſilver. About one ounce-meaſure was quite tranſparent, but preſently after it became exceedingly red; and being ſatisfied that this redneſs was owing to the nitrous acid vapour having diſſolved the quickſilver, I took no more than two ounce-meaſures in this way, but received all the remainder, which was almoſt two pints, in water. Far the greateſt part of this was fixed air, being readily abſorbed by water, and extinguiſhing a candle. There was, however, a conſiderable reſiduum, in which the flame of a candle burned with a crackling noiſe, from which I concluded that it was true dephlogiſticated air.

In this experiment I had moiſtened the red lead with ſpirit of nitre ſeveral times, and had dried it again. When I repeated the experiment, I moiſtened it only once with the ſame acid, when I got from it not quite a pint of air; but it was almoſt all of the dephlogiſticated kind, about five times as pure as common air. N. B. All the acids made a violent effervеſcence with the red lead.

Though there was a difference in the reſult of theſe experiments, which I ſhall conſider hereafter, I was now convinced that it was the nitrous acid which the red lead had acquired from the air, and which had enabled it to yield the dephlogiſticated air, agreeable to my original conjecture. Finding alſo, as will be ſeen in the following ſection, that the ſame kind of air is produced by moiſtening with the ſpirit of nitre any kind of earth that is free from phlogiſton, and treating it as I had done the red lead in the laſt-mentioned experiment, there remained no doubt in my mind, but that *atmoſpherical air*, or the thing that we breathe, *conſiſts of the nitrous acid and earth*, with ſo much phlogiſton as is neceſſary to its elaſticity; and likewiſe ſo much more as is required to bring it from its ſtate of perfect purity to the mean condition in which we find it.

For this purpoſe I tried, with ſucceſs, *flowers of zinc*, *chalk*, *quick-lime*, *ſlacked-lime*, *tobacco-pipe clay*, *flint* and *Muſcovy talck*, with other ſimilar ſubſtances, which will be found to comprize all the kinds of earth that are eſſentially diſtinct from each other, according to their chymical properties. A particular account of the proceſſes with theſe ſubſtances, I reſerve for another ſection; thinking it ſufficient in this to give a hiſtory of the diſcovery, and a general account of the nature of this dephlogiſticated air, with this general inference from the experiment, reſpecting the conſtitution of the atmoſphere.

I was the more confirmed in my idea of ſpirit of nitre and earth conſtituting reſpirable air, by finding, that when any of theſe matters, on which I had tried the experiment, had been treated in the manner above mentioned, and they had thereby yielded all the air that could be extracted from them by this proceſs; yet when they had been moiſtened with freſh ſpirit of nitre, and were treated in the ſame manner as before, they would yield as much dephlogiſticated air as at the firſt. This may be repeated till all the earthy matter be exhauſted. It will be ſufficient to recite one or two facts of this kind from my regiſter.

April 18, I took the remains of the freſh made red lead, out of which a great quantity of dephlogiſticated air had been extracted, and moiſtening about three quarters of an ounce of it a ſecond time with ſpirit of nitre, I got from it about two pints of air, all of which was nearly ſix times as pure as common air. This air was generated very faſt, and the glaſs tube through which it was tranſmitted was filled with red fumes; the nitrous acid, I ſuppoſe, prevailing in the compoſition of the air, but being abſorbed by the water in which it was afterwards received.

In this, and many other proceſſes, my reader will find a great variety in the purity of the air procured from the ſame ſubſtances. But this will not be wondered at, if it be conſidered that a ſmall quantity of phlogiſtic matter, accidentally mixing with the ingredients for the compoſition of this air, depraves it. It will alſo be unavoidably depraved, in ſome meaſure, if the experiment be made in a gun-barrel, which I commonly made uſe of, when, as was generally the caſe, it was ſufficiently exact for my purpoſe, on account of its being the eaſieſt, and in many reſpects, the moſt commodious proceſs.

The reaſon of this is, that if the produce of air be not very rapid, there will be time for the phlogiſton to be diſengaged from the iron itſelf, and to mix with the air. Accordingly I have ſeldom failed to find, that when I endeavoured to get all the air I poſſibly could from any quantity of materials, and received the produce at different times (as for my ſatisfaction I generally did) the laſt was inferior in purity to that which came firſt. Not unfrequently it was phlogiſticated air; that is, air ſo charged with phlogiſton, as to be perfectly noxious; and ſometimes, as the reader will find in the next ſection, it was even nitrous air.

On the ſame account it frequently happened, that when I uſed a conſiderable degree of heat, the red lead which I uſed in theſe experiments would be changed into real lead, from which it was often very difficult to get the gun-barrel perfectly clear.

A good deal will alſo depend upon the ingredients which have been uſed in the gun-barrel in preceding experiments: for it is not eaſy to get ſuch an inſtrument perfectly clean from all the matters that have been put into it: and though it may be preſumed, in general, that every kind of air will be expelled from ſuch ingredients by making the tube red-hot; yet matters containing much phlogiſton, as charcoal, &c. will not part with it in conſequence of the application of heat, unleſs there be at hand ſome other ſubſtance with which it may combine. Though, therefore, a gun-barrel, containing ſuch ſmall pieces of charcoal as cannot be eaſily wiped out of it, be kept a long time in a red heat, and even with its mouth open; yet if it be of a conſiderable length, ſome part of the charcoal may remain unconſumed, and the effect of it will be found in the ſubſequent experiment. Of this I had the following very ſatisfactory proof.

Being deſirous to ſhew ſome of my friends the actual production of dephlogiſticated air, and having no other apparatus at hand, I had recourſe to my gun-barrel; but apprized them, that having uſed it the day before to get air from charcoal, with which it had been filled for that purpoſe, though I had taken all the pains I could to get it all out, yet ſo much would probably remain, that I could not depend on the air I ſhould get from it being dephlogiſticated; but that it would probably be of an inferior quality, and perhaps even nitrous air. Accordingly, having put into it a mixture of ſpirit of nitre and red lead (being part of a quantity which I had often uſed before for the ſame purpoſe) dried, and pounded, I put it into the fire, and received the air in water.

The firſt produce, which was about a pint, was ſo far nitrous, that two meaſures of common air, and one of this, occupied the ſpace of little more than two meaſures; that is, it was almoſt as ſtrongly nitrous as that which is produced by the ſolution of metals in ſpirit of nitre. The ſecond pint was very little different from common air, and the laſt produce was better ſtill, being more than twice as good as common air. If, therefore, any perſon ſhall propoſe to make dephlogiſticated air, in large quantities, he ſhould have an apparatus appropriated to that purpoſe; and the greateſt care ſhould be taken to keep the inſtruments as clear as poſſible from all phlogiſtic matter, which is the very bane of purity with reſpect to air, they being exactly *plus* and *minus* to each other.

The hypotheſis maintained in this ſection, viz. that atmoſpherical air conſiſts of the nitrous acid and earth, ſuits exceedingly well with the facts relating to the production of nitre; for it is never generated but in the open air, and by expoſing to it ſuch kinds of earth as are known to have an affinity with the nitrous acid; ſo that by their union common nitre may be formed.

Hitherto it has been ſuppoſed by chymiſts, that this nitrous acid, by which common nitre is formed, exiſts in the atmoſphere as an *extraneous ſubſtance*, like water, and a variety of other ſubſtances, which float in it, in the form of effluvia; but ſince there is no place in which nitre may not be made, it may, I think, with more probability be ſuppoſed, according to my hypotheſis, that nitre is formed by a real *decompoſition of the air itſelf*, the *baſes* that are preſented to it having, in ſuch circumſtances, a nearer affinity with the ſpirit of nitre than that kind of earth with which it is united in the atmoſphere.

My theory alſo ſupplies an eaſy ſolution of what has always been a great difficulty with chymiſts, with reſpect to the *detonation of nitre.*

nitre. The queſtion is, what becomes of the nitrous acid in this caſe? The general, I believe the univerſal, opinion now is, that it is *deſtroyed*; that is, that the acid is properly decompoſed, and reſolved into its original elements, which Stahl ſuppoſed to be earth and water. On the other hand, I ſuppoſe that, though the common properties of the acid, as combined with water, diſappear, it is only in conſequence of its combination with ſome earthy or inflammable matter, with which it forms ſome of the many ſpecies of air, into the compoſition of which this wonderful acid enters. It may be common air, it may be dephlogiſticated air, or it may be nitrous air, or ſome of the other kinds, of which an account will be given in a ſubſequent ſection. That it ſhould really be the nitrous acid, though ſo much diſguiſed by its union with earthy, or other matters, will not appear extraordinary to any perſon who ſhall conſider how little the acid of vitriol is apparent in common ſulphur.

With reſpect to *mercurius calcinatus*, and *red lead*, their red colour favours the ſuppoſition of their having extracted ſpirit of nitre from the air.

SECTION IV.

A more particular Account of ſome Proceſſes for the Production of Dephlogiſticated Air.

I cannot promiſe thoſe of my readers, whoſe object is nothing more than *general information*, much pleaſure from the peruſal of this ſection, as it will conſiſt, for the moſt part, of a dry detail of proceſſes, for procuring dephlogiſticated air; but as they all appeared neceſſary, in my inveſtigation of the ſubject, I doubt not but an attention to them will be of uſe to ſuch as are diſpoſed to purſue the inquiry themſelves. I might have contented myſelf with giving a general idea of the reſult of ſuch experiments; but that would have been to mix my own *opinions* with *facts*, in ſuch a manner that the reader would not have been able to ſeparate them. At preſent, if I ſhould be miſtaken in any of my opinions, the reader, having before him all the facts on which thoſe opinions were grounded, will be able to rectify the miſtake, and prevent the error from ſpreading.

Having ſeen ſufficient reaſon to conclude that reſpirable air conſiſts of nitrous acid and earth, my object, in all this courſe of experiments,

ments, was ſimply to find *what kind of earth* was moſt proper for this purpoſe, or which had the moſt aptneſs to form this peculiar union with the nitrous acid. Upon the whole, I think it will appear that the *metallic earths*, if they be free from phlogiſton, are the moſt proper, and next to them the *calcareous* earths; but that a very great difference in the production of this kind of air depends upon a variety of circumſtances in which the experiments are made.

I have obſerved that *red lead*, without any addition, yields dephlogiſticated air by heat. To give ſome idea of the differences in the reſults, from what is, to appearance, the ſame preparation, and of the conſequence of adding ſpirit of nitre to the red lead, I muſt inform my reader, that having weighed two half-ounces of red lead, taken from the ſame parcel, I put one of them, without any addition, into the gun-barrel, and with a very briſk fire (which is generally a conſiderable advantage for the production of air) I got no more than three ounce-meaſures, and it was very little better than common air.

The ſecond half-ounce I moiſtened with a very diluted ſpirit of nitre; and when it was dried and pounded, I put it into the ſame gun-barrel; and, in the ſame circumſtances with the former, I got from it about three pints of air, the firſt part of which was ſo far dephlogiſticated, that two meaſures of it, and five of nitrous air, occupied the ſpace of two meaſures only; of the ſecond quantity, two meaſures were not increaſed by the addition of ſeven meaſures of nitrous air. This was the pureſt air that I had then ſeen. The laſt produce was almoſt all pure fixed air, being not at all affected by nitrous air. extinguiſhing a candle, and precipitating lime in lime-water. It was, indeed, a little of a nitrous nature; for it diminiſhed common air in a ſmall degree, an effect which I attribute to the phlogiſton coming from the iron.

A remarkable difference in the quantity of the produce of this kind of air, as I hinted juſt now (and as I have obſerved in a former publication, in the produce of inflammable air) depends upon the *ſuddenneſs* with which the ſame degree of heat is applied. The following muſt be reckoned a remarkable fact of this kind, and it was made with as much care as I could poſſibly apply. From an ounce of red lead, by a ſudden and briſk heat, I got above two quarts of air, a great part of which was fixed air, and the reſt was about twice as good as common air; and immediately after, putting the very ſame quantity of the ſame parcel of red lead into the ſame gun-barrel, by heat very *ſlowly applied*, but urged vehemently at laſt, I got no more than two ounce-meaſures of air, a great part of which was fixed air, and the reſt not ſo good as common air.

I had been told that red lead acquired additional weight by being often waſhed in water. In order to try whether this was the fact, and alſo whether the red lead acquired its property of yielding dephlogiſticated air by this means, I waſhed a quantity of that parcel which I had got freſh-made, four times in diſtilled water, evaporating it to dryneſs each time; but no more air came from it than when it had not been wetted, neither was it at all increaſed in weight by this means.

I have obſerved that, in general, thoſe ſubſtances which, without containing phlogiſton, yield fixed air with heat, or by the addition of an acid, when mixed with ſpirit of nitre, and treated as above, yield more or leſs of dephlogiſticated air; but generally with a conſiderable mixture of fixed air, though I profeſs not to know upon what circumſtance it is that the proportion of theſe two kinds of air, produced from theſe ſubſtances, depends, With a very ſmall degree of heat, *white lead*, without any addition, yields a very great quantity of pure fixed air. Having moiſtened about an ounce-meaſure of it with ſpirit of nitre, and having put it into a glaſs-veſſel, with a ground-ſtopple and tube, I extracted from it, at five different times, five pints of air, each of which I examined ſeparately, as uſual, and the reſults were as follows.

Of the firſt quantity, about $\frac{19}{20}$ or $\frac{29}{30}$ was abſorbed by water, and the remainder neither affected common air, nor was affected by nitrous air, ſo that it was pure fixed air; and conſidering the quantity that would be neceſſarily abſorbed by the water in which it was received, before I made any trial of the properties of it, it may perhaps be deemed to have been as free from any foreign mixture as any that was ever procured. Of the ſecond quantity, about twice as much was left unabſorbed by water, and the reſiduum appeared to be dephlogiſticated; for it took about an equal meaſure of nitrous air to ſaturate it; and conſequently it was nearly twice as good as common air. Of the third quantity, as little remained unabſorbed by water as of the firſt; but the reſiduum was as pure as that of the ſecond quantity. Of the fourth quantity, one-fourth remained unabſorbed by water, and it took $1\frac{3}{4}$ of nitrous air to ſaturate it. Of the fifth pint, one-half remained unabſorbed, and it took more than two equal meaſures of nitrous air to ſaturate it; ſo that it was nearly four times as pure as common air. Laſtly, a ſingle ounce-meaſure, that came very ſlowly after the five pints, was no part of it abſorbed by water, and it took $1\frac{1}{2}$ of nitrous air to ſaturate it, being about three times as pure as common air.

From a quantity of *litharge*, moiſtened with ſpirit of nitre, and dried, I got, in a gun-barrel, a great quantity of air; about half of which was fixed air, precipitating lime in lime-water, and the other half was ſtrongly nitrous; but with a burning lens, in quickſilver, I got a very pure dephlogiſticated air from this mixture.

To go through the different ſtates of lead in this manner, I took half an ounce of *lead-ore*, and having ſaturated it with ſpirit of nitre, I dried it as before, put it into a gun-barrel, filled up to the mouth with pounded flint, and placed veſſels filled with water to receive the air. The conſequence was, that as ſoon as this mixture began to be warm, air was generated very faſt, inſomuch that, being rather alarmed, I ſtood on one ſide; when preſently there was a violent and loud exploſion, by which all the contents of the gun-barrel were driven out with great force, daſhing to pieces the veſſels that were placed to receive the air, and diſperſing the fragments all over the room; ſo that all the air which I had collected, and which was about a pint, was loſt. The mixture, before it was put into the gun-barrel, was betwixt white and yellow, and had very much the ſmell of brimſtone; ſo that it was in fact a compoſition ſimilar to gun-powder.

Being deſirous to know what kind of air I had got by this proceſs, I put the ſame materials into a glaſs-phial, and putting it into a crucible with ſand, diſpoſed the apparatus for receiving the air in ſuch a manner, that the exploſion could not affect it. It did explode as before, but the air was preſerved, and appeared to be very ſtrong nitrous air, almoſt as much ſo as that which is procured by the ſolution of metals.

From the *grey calx of lead*, treated in the ſame manner, I got about a pint of air, half of which, being readily abſorbed by water, I take for granted, was fixed air; but the remainder was ſtrongly nitrous. Had I not waſhed it a good deal in water, it would pro-

bably

bably have been as ſtrong as that which is procured from metals.

The pureſt air that I ever procured was from *flowers of zinc*, moiſtened, as in the other proceſſes, with ſpirit of nitre, and put into a glaſs-phial, with a ground-ſtopple and tube. At firſt I deſpaired of getting any air at all from the proceſs; but at length it came in a prodigious torrent, and was ſo cloudy, that the burſting of every bubble, after it had paſſed through the water, reſembled the burſting of a bag of flour. The tube through which it was tranſmitted was exceedingly red, and in ſome degree, the inſide of the receiver too, as might be perceived amidſt the thick cloud that filled it. This cloudineſs of the newly-generated air, I have often perceived in the proceſs with red-lead, but never in ſo great a degree as in this caſe.

The quantity of air procured was nearly three pints, from about half an ounce-meaſure of the flowers of zinc; and it was ſo highly dephlogiſticated, that it took three times its bulk of nitrous air before its dimenſions were increaſed. When it had got only twice its bulk of nitrous air, it was reduced to leſs than one-fifth of its original quantity. The laſt produce came very ſlowly, and was not quite ſo pure. The flowers of zinc, which I uſed in this experiment, I had from Dr. Higgins. They formed a very hard and brittle ſubſtance, when mixed with ſpirit of nitre, and dried. After the proceſs it ſwelled, and broke the phial into many pieces.

Beſides theſe, I tried no earth of any metal except the *ruſt of iron* and *white arſenic*, both of which, when treated in the manner above mentioned, and put into a gun-barrel, yielded nothing but fixed or nitrous air; ſo that theſe calces undoubtedly contained much phlogiſton, and the flowers of zinc, perhaps, none at all.

From conſiderably leſs than half an ounce of *ruſt of iron*, moiſtened with ſpirit of nitre, and dried, I got about a quart of air, about one-third of which was fixed air, precipitating lime in lime-water, &c. and the remainder was nitrous; ſo that two meaſures of common air and one of this, occupied the ſpace of leſs than two meaſures.

The *white arſenic* I procured from Dr. Higgins, who aſſured me that it contained the leaſt phlogiſton poſſible. I moiſtened about an ounce-meaſure of it with ſpirit of nitre, and putting it into a phial with a ground-ſtopple and tube, with no great degree of heat, I extracted

I extracted from it four ounce-meaſures of air, and it was as ſtrongly nitrous as any that I had ever procured from metals. I increaſed the heat till the phial was melted, without getting any more air. The tube was exceedingly red during the tranſmiſſion of the air through it.

Next to the metallic earths of lead and zinc, I found the *calcareous* earths the moſt proper for the production of dephlogiſticated air; but I had no opportunity of trying any great variety of them. The beſt that I did try was *chalk*. Having ſaturated half an ounce of it with diluted ſpirit of nitre, and dried it, I got from it, in a gun-barrel, more than a pint of air, which was highly dephlogiſticated. I began to receive this produce in quickſilver, the conſequence of which was, that the nitrous acid, coming over in the form of vapour, diſſolved the quickſilver, and made nitrous air; but a cruſt being formed upon the ſurface of it, prevented the ſolution of more, and the air continued red a long time.

From another ounce-meaſure of chalk, treated in the ſame manner, I got about a quart of air. What I took firſt was conſiderably nitrous, two meaſures of common air and one of this, occupying the ſpace of 2 $\frac{1}{2}$ meaſures. The ſecond pint was dephlogiſticated; ſo that two meaſures of it, and five of nitrous air, occupied the ſpace of two meaſures. The laſt was leſs dephlogiſticated, being about one-half better than common air. At this time the air was generated with prodigious rapidity; the glaſs-tube through which it was tranſmitted was exceedingly red; and when, in changing the veſſels, ſome of the vapour eſcaped into the air, it had the reddeſt appearance of any thing that I had ever ſeen of the kind.

Having ſaturated half an ounce of exceedingly good *quick-lime* with diluted ſpirit of nitre, dried it, and put it into a gun-barrel, I got from it about a pint and half of air, the firſt part of which was ſo far dephlogiſticated, that it required an equal meaſure of nitrous air to ſaturate it. The ſecond was no better than common air, and the third was equal to the firſt. In this proceſs the air was produced very irregularly, ſometimes coming in great quantities, and at other times the water would ruſh back into the tube.

I repeated the experiment on quick-lime, in a glaſs-phial and tube, when the whole quantity was ſo pure, that it required twice its bulk of nitrous air to ſaturate it. The produce of air, in this experiment, was as irregular as in the preceding. I could have wiſhed to have treated the ſtone from which that lime was made in the ſame manner, but I had no opportunity.

From *lime fallen in air*, moiſtened with ſpirit of nitre, and treated as above, in a gun-barrel, I got near a pint of air, the greateſt part of which came very rapidly, the fire being urged very much; and it was ſo far dephlogiſticated, that two meaſures of it required five meaſures of nitrous air to ſaturate it. The laſt produce came very ſlowly, and it was ſo far nitrous, that two meaſures of common air, and one of it, occupied the ſpace of leſs than two meaſures; that is, it was very nearly perfectly nitrous.

I alſo moiſtened with ſpirit of nitre a quantity of *lime that had been plunged in water*, in order to make lime-water, and got air from it in a gun-barrel, very irregularly, as before: one part of this air, which came almoſt at once, was dephlogiſticated, ſo that two meaſures of this, and five of nitrous air occupied the ſpace of 2 ½ meaſures.

From two ounce-meaſures of pounded *marble*, treated as above, in a gun-barrel, I got about three quarts of air; but a very great proportion of it was fixed air, eſpecially the laſt produce, which indeed was very little elſe; but towards the beginning of the proceſs, the reſiduum was a little better than common air.

Repeating this experiment on marble, in a glaſs-phial with a ground-ſtopple and tube, I extracted from an ounce-meaſure of it about two pints of air, the greateſt part of which was ſo highly dephlogiſticated, that it took nearly three equal quantities of nitrous air to ſaturate it. Even the laſt produce was hardly to be diſtinguiſhed from the firſt. What remained in the phial after the experiment ſwelled and broke it.

From *magneſia*, both calcined and uncalcined, I got, in a gun-barrel, a conſiderable quantity of air. From the calcined magneſia it was not much better than common air; from the uncalcined it was more than twice as good. But very probably this difference may not be invariable.

I think it very probable that dephlogiſticated air may be procured from any kind of earth with which the ſpirit of nitre will unite; eſpecially if it will likewiſe admit of a combination with fixed air or alkaline air, ſo that the ſpirit of nitre muſt expel the fixed air or alkaline air, before it can incorporate with it. Of ſubſtances of theſe kinds, beſides thoſe above mentioned, I tried *ſalt of tartar* and *wood-aſhes.*

From half an ounce of *ſalt of tartar*, moiſtened with ſmoking ſpirit of nitre, and dried, I got, in a gun-barrel, about half a pint of air, the greateſt part of which was fixed air, with a reſiduum ſo far dephlogiſticated, as to be about three times as good as common air. The produce of air, in this experiment, was not very rapid, and it continued a long time. More would have been collected; but that part of it eſcaped at the luting.

I moiſtened about half an ounce-meaſure of *aſhes of wood*, carefully burned, firſt in an iron-ladle, and then in a crucible, with ſtrong ſmoking ſpirit of nitre; and, in a gun-barrel, I got from it about three pints of air, part of which was fixed, precipitating lime in lime-water, &c. and the reſt was ſo pure, that it abſorbed nearly three times its bulk of nitrous air. The laſt produce was very ſlow, and only about twice as good as common air.

From an ounce-meaſure of aſhes of *pit-coal*, burned with all poſſible care, and treated in the ſame manner as above, I got about three quarts of air, one-third of which was fixed air, precipitating lime in lime-water; but the reſiduum was ſtrongly nitrous, eſpecially at the laſt. It is obſervable enough, from their dark colour (of which no burning, I believe, will diveſt them) that, in general, aſhes of pit-coal contain much more phlogiſton than aſhes of wood.

N. B. In this, and other proceſſes, it will be obſerved, that fixed air was procured from ſubſtances, which can hardly be thought to have contained it before: which affords a preſumption, that that it is not an acid *ſui generis*, but a modification of the nitrous acid.

Clay is a ſubſtance altogether different from calcareous earth, and is not ſuppoſed, I believe, to contain any air at all. Of that ſpecies of it, which is called *tobacco-pipe-clay*, and which, I believe, is the pureſt of all, I got from Dr. Higgins, a quantity in powder; and moiſtening it with ſpirit of nitre, I obſerved that no more heat or efferveſcence was produced than the mixing of it with water would have occaſioned.

Putting it, when dry, into a gun-barrel, I extracted from it, by a ſtrong heat, about two ounce-meaſures of air, which being pretty readily abſorbed by water, neither affecting common air, nor being affected by nitrous air, and extinguiſhing a candle, I concluded to be fixed air. Repeating the experiment, I got the ſame produce, only obſerving, that the fixed air made lime-water turbid, the moſt certain teſt, I believe, of the preſence of fixed air; and the laſt produce was highly nitrous. Imagining that this produce might have come from the phlogiſton of the iron, I reſolved to repeat the experiment once more, with all poſſible care, in a glaſs-phial, with a ground-ſtopple and tube.

I did ſo, and took the produce at eight different times. The firſt and ſecond quantities had a good deal of fixed air in them; the reſiduum of the firſt was a little diminiſhed by nitrous air, almoſt as much as air in which a candle had burned out, which might be owing, in part, to the common air contained in the phial. The reſiduum of the ſecond quantity, on the other hand, diminiſhed common air a little; ſo that two meaſures of common air, and one of this, occupied the ſpace of 2 $\frac{1}{3}$ meaſures. Of the third quantity, two meaſures required three meaſures of nitrous air to ſaturate it; ſo that it was pretty highly dephlogiſticated. Of the fourth, two meaſures, and three of nitrous air, occupied the ſpace of 1 $\frac{3}{4}$ meaſures. The fifth was of the ſame quality with the third. The ſixth required twice its quantity of nitrous air to ſaturate it. The ſeventh was not quite ſo pure as the ſixth; and the eighth neither affected common air, nor was affected by nitrous air, being what I term *phlogiſticated air*. As ſome part of this produce was nitrous air, it is evident that the phlogiſton neceſſary to conſtitute it muſt have been in the clay, and not in the veſſel containing it, which was of glaſs.

Having by me a quantity of *Stourbridge clay*, I had the curioſity to repeat the experiments with this ſpecies, to ſee whether there would be any material difference in the reſult. Uſing the gun-barrel, I received the air in four portions. The firſt was fixed air, making lime-water turbid, and being more than half-abſorbed by water; the ſecond was about as good as common air, and the fourth was conſiderably nitrous.

To avoid the effect of the gun-barrel, I then put the clay into a phial, with a glaſs-ſtopple and tube, and putting it into a ſand-heat, I received the air, for the ſake of greater exactneſs, in ten different portions, about one-half of an ounce-meaſure each. The firſt produce was half-abſorbed by water, with a reſiduum ſo far nitrous, that two meaſures of common air and one of this, occupied the ſpace of 2 $\frac{1}{2}$ meaſures. The ſecond and third portions were almoſt wholly fixed air, precipitating lime in lime-water, and not at all affecting common air, or being affected by nitrous air. Of the fourth I have no account. The fifth was ſo far dephlogiſticated, that two meaſures of it, and three of nitrous air, occupied the ſpace of 2 $\frac{1}{2}$ meaſures. The ſixth and ſeventh produce were, as nearly as poſſible, common air. The ninth was ſo far nitrous, that two meaſures of common air, and one of this, occupied the ſpace of 2 $\frac{1}{2}$ meaſures; and the tenth diminiſhed common air ſtill leſs.

It is evident, from the courſe of this proceſs, that phlogiſton muſt have been contained in the clay, and have been diſengaged at different times, according as the heat affected different parts of the mixture. Had the whole of this produce been taken together, it would have been about the ſtandard of common air mixed with fixed air; which ſhews the importance of taking the produce in different veſſels, and examining them ſeparately; a practice which the reader will find I have often had recourſe to with great advantage.

As a great degree of heat cannot be applied to any thing contained in a glaſs-veſſel, with-without melting it, and I was willing to know what would be the effect of more heat on this very clay when the above mentioned experiment was over, I took it out of the phial, and put it into a gun-barrel; when I got a conſiderable quantity of air from it. Part of it was fixed air, precipitating lime in lime-water, and the remainder was like the reſiduum of fixed air, or phlogiſticated common air, extinguiſhing a candle, and being neither affected by nitrous air, nor affecting common air. I did the ſame thing with the *tobacco-pipe-clay*, which remained after the experiment above recited, and had nearly the ſame reſult. The firſt produce was of the ſame degree of purity with common air, and the next was a little affected by nitrous air.

From a quantity of *gypſum*, which I procured in the form of powder, I got a quantity of fixed air in a gun-barrel; and from the ſame, moiſtened with ſpirit of nitre, and treated in the ſame manner, I got a little fixed air, with a great proportion of nitrous air, almoſt as ſtrong as any. But ſuſpecting that this gypſum was not pure, I got of Dr. Higgins a piece of that kind of which the fineſt plaiſter is made, and from this, mixed with ſpirit of nitre, I got a conſiderable a conſiderable quantity of air, part of which was fixed air, and the remainder neither affected common air, nor was affected by nitrous air, and extinguiſhed a candle. At laſt the air was nitrous, as I ſuſpect, from the gun-barrel.

Being rather ſurprized that this kind of earth, which had the appearance of being very free from phlogiſton, ſhould yield air of no better quality than this, I repeated the experiment, by taking the produce of air at ſeveral times, as in former experiments, moiſtening the earth with a ſtronger ſpirit of nitre than before; and inſtead of a gun-barrel, made uſe of a phial with a ground-ſtopple and tube. The quantity of air produced in this manner, was about two ounce-meaſures, from an ounce-meaſure of the plaiſter, and I received the air in four different parts.

The firſt was a little diminiſhed by nitrous air, being, I ſuppoſe, in a great meaſure, common air not quite expelled from the phial; and the ſecond was ſtrong nitrous air, perhaps from ſome phlogiſtic matter accidentally mixed with the ingredients. I am the more induced to think ſo, becauſe the third and fourth produce was ſo highly dephlogiſticated, that one meaſure of each took five meaſures of nitrous air to ſaturate it; ſo that they were each four times as good as common air.

After the preceding experiments, there remained only the *cryſtalline* and *talcky earths*, that are eſſentially different from each other; and each of theſe alſo yielded dephlogiſticated air, when they were treated in the ſame manner with the earths above mentioned, but in a very ſmall quantity.

When I took common *flints*, as they are dug out of the ground, part white and part black, moiſtening the powder of them with ſpirit of nitre, as before, and uſing a gun-barrel, I got fixed air, with a great proportion of nitrous air; that which came over the firſt being like the reſiduum of fixed air, extinguiſhing a candle, but being not readily abſorbed by water.

After this I got ſome *flints carefully calcined in cloſe veſſels*, by Dr. Higgins, and having pounded a quantity of them, and moiſtened the powder with ſpirit of nitre, I put it into a glaſs-phial with a ground ſtopple and tube; and applying, at firſt, the flame of a candle only, the air I got was in a very ſmall quantity; but it precipitated lime in lime-water, and diminiſhed common air a little.

I then

I then put the ſame apparatus into a ſand-heat, when I got, in all, as much air as twice the bulk of the materials. Part of it precipitated lime in lime-water, but the reſt of the produce was conſiderably better than common air; and the laſt was ſo good, that it took two meaſures of nitrous air to ſaturate it.

Leſt this air might come from ſome extraneous matter, mixed with the powder of flint, I put ſome freſh ſpirit of nitre upon the ſame materials, without taking them out of the phial, after I had found that they would yield no more air from the firſt proceſs, and I replaced the phial in the ſame ſand-heat. The air firſt produced in this ſecond proceſs was but little diminiſhed by nitrous air, but the reſt was almoſt as pure as any that I had ever got before. The quantity of it, however, was not more than the bulk of the materials.

N. B. When, in this experiment, the bubbles of air burſt, after getting through the water, a whitiſh cloud iſſued from them, as in the rapid production of nitrous air, and as in the produce of dephlogiſticated air from the flowers of zinc above mentioned, but in a much leſs degree.

I repeated the ſame proceſs not leſs than half a dozen times, putting freſh ſpirit of nitre upon the ſame materials, without taking them out of the phial, but the reſult was always the ſame; the firſt produce of air being always phlogiſticated, then (after an interval in which nothing but the pure vapour of ſpirit of nitre came over) the remainder being the dephlogiſticated air above mentioned.

To complete this courſe of experiments, I, in the laſt place, put ſtrong ſpirit of nitre into a phial, filled with tranſparent *Muſcovy talc*, ſuch as opticians make uſe of for confining microſcopic objects. In this proceſs every thing went on in the very ſame manner as with the calcined flint; the firſt produce being phlogiſticated air, or air of ſuch quality as neither to affect common air, nor be affected by nitrous air, then the pure vapour of the ſpirit of nitre; and laſtly, about an ounce-meaſure of air, about five times as good as common air. The pieces of talc which had been contiguous to the ſides of the phial appeared to be a little whitened after the experiment, but the reſt looked as if they had never been uſed in that manner; being as tranſparent as before, and of as firm a texture, but ſeemingly more flexible; ſo that thoſe pieces, when handled all together, felt like ſoft feathers.

It is ſufficiently evident from theſe experiments, that dephlogiſticated air is produced from all kinds of earth mixed with ſpirit of nitre, only that a greater quantity of air is produced from ſome than from others; the advantage in this reſpect being on the ſide of the metallic and calcareous earths.

I would obſerve, that this proceſs ſeems to furniſh a pretty accurate teſt, perhaps the moſt accurate hitherto known, of the preſence of phlogiſton in bodies. Perhaps no ſpecies of air can be produced without a certain portion of phlogiſton; but probably the nitrous acid itſelf always contains ſufficient for the purpoſe of dephlogiſticated air. But nitrous air contains ſo much phlogiſton, that I think it cannot be produced unleſs the materials themſelves contain it in a very conſiderable degree. Thus I have no doubt but that *white arſenic*, though it may be thought to contain no phlogiſton, really does contain a conſiderable quantity of it; whereas, if the air be highly dephlogiſticated, I think it may be conſidered as the moſt ſatisfactory proof we are yet acquainted with, that the ſubſtance contains no phlogiſton at all.

I ſhall cloſe this ſection with an account of the extraction of pure air from other ſubſtances beſides *mercurius calcinatus*, and *red lead*, without the addition of ſpirit of nitre. Of this kind I have only found two ſubſtances, viz. *ſedative* ſalt, and *Roman vitriol ſlightly calcined*, which I had from Dr. Higgins; beſides common *ſalt-petre*, which is known to contain the acid of nitre in itſelf. From the two former I extracted air by means of a burning lens in quickſilver.

The *ſedative ſalt* is not very manageable in this proceſs; but, with ſome difficulty, I did extract from it a ſmall quantity of air, in which a candle burned as in common air, and which was diminiſhed as much as common air by nitrous air. At another time, the air which I extracted from this ſubſtance was not diminiſhed by nitrous air quite ſo much as common air is.

N. B. The quantity of air was always very ſmall, not more than the bulk of the materials.

From the *Roman vitriol* I alſo got but a ſmall quantity of air. The firſt that I got was diminiſhed by nitrous air, exactly as much as common air. I repeated the experiment, and the air which I then got was diminiſhed by nitrous air conſiderably more than common air.

The reſult of theſe experiments rather ſurprizes me, as, after many trials made with a view to it, I could get no ſuch air from any ſpecies of *factitious vitriol*, calcined or uncalcined. There muſt certainly have been ſome nitrous acid in that Roman vitriol.

The readers of my former publications on this ſubject will remember, that I was exceedingly puzzled with the experiments which I made to extract air from *ſalt-petre* in a gun-barrel; the reſults appearing to me very extraordinary, and well worth attending to, as they might lead to conſiderable diſcoveries. See Vol. I. p. 155. In fact, there was ſufficient reaſon for the conjecture; but the method which I then took to extract air from this ſubſtance was ill adapted to make it yield its genuine produce. I had not, however, at that time, thought of any other.

The air I firſt got admitted a candle to burn in it with a very ſtrong flame, and with a crackling noiſe. Alſo, though, after having ſtood a whole year in water, it became quite noxious, yet by agitation in freſh water, it was perfectly reſtored; ſo that a candle would burn in it again. At the time of my laſt publication, I conjectured that this air was *phlogiſticated nitrous air*; but now I think it muſt have been deplogiſticated air, though produced in a gun-barrel, in which the ſpirit of nitre, by diſſolving the iron, would be very apt to deprave the air; and accordingly, in repeating this experiment ſome time afterwards, I got air that extinguiſhed a candle.

I was much puzzled, at that time, to account for the very different reſults of what was, to appearance, the ſame experiment; but I do not wonder at it now. I imagine that, in the former caſe, the air was produced very rapidly, and therefore that there was not time for the ſpirit of nitre to act upon the iron; and conſequently the ſalt-petre gave its natural produce: whereas, in the latter caſe, a mixture of nitrous air (produced by the ſolution of iron in the nitrous acid, diſengaged from the ſalt-petre) had thoroughly depraved the air. I advance this with the more certainty, as I have found that ſalt-petre, heated in a glaſs-veſſel, yields very pure dephlogiſticated air, its own earth, and the ſpirit of nitre which it contains, being capable, by heat, of forming that kind of union of thoſe two principles which the conſtitution of that air requires; and this, I think, is a pretty remarkable circumſtance.

It may be worth while to obſerve, that I began my experiments upon nitre in quickſilver;

ſilver; but that the air produced in this manner was nitrous, occaſioned by the ſolution of the quickſilver, as in the former caſe, by the ſolution of the iron in the ſpirit of nitre diſengaged in the operation. A copious white fume iſſued from the nitre in the courſe of this experiment, like that which attends the rapid production of nitrous air from metals.

When I had recourſe to my tall glaſs-veſſels (fig. *d.*) I uſed an ounce of ſalt-petre pounded; and filling the veſſel up to the mouth with pounded flint, I took the produce of air at nine times, each about three quarters of an ounce-meaſure. The firſt produce was not quite ſo good as common air, the ſecond was of the ſame degree of purity with common air, the third rather worſe; but the fourth was ſo far dephlogiſticated, that one meaſure of it, and two of nitrous air, occupied the ſpace of one-fifth leſs than one meaſure. The fifth produce was ſtill better; for one meaſure of it, and two of nitrous air, occupied the ſpace of half a meaſure. The ninth was about the ſame degree of purity; and the reſt, I preſume, were not much different.

Being deſirous of knowing what kind of air was produced by the exploſion of gun-powder, I, for that purpoſe, mixed equal quantities of brimſtone,

brimſtone and ſalt-petre, both finely pounded, and put them into a tall glaſs-veſſel. The production of air was very rapid and copious, and ſo highly nitrous, that two meaſures of common air, and one of this, occupied the ſpace of 2 $\frac{1}{4}$ meaſures. Since the produce of air from ſpirit of nitre and charcoal is the very ſame with this, viz. nitrous air, it cannot be doubted but that nitrous air is alſo produced in the exploſion of gun-powder, which is compoſed of thoſe ingredients; the ſpirit of nitre not being deſtroyed, or ſo far decompoſed as that its acid nature is loſt, but only entering into the compoſition of this ſpecies of air.

SECTION V.

Miſcellaneous Obſervations on the Properties of DEPHLOGISTICATED AIR.

I endeavoured, in a variety of ways, to find the ſpecific gravity of dephlogiſticated air, by carefully weighing the materials before and after the production; and though this is by no means an exact method of aſcertaining this circumſtance, and I had recourſe to better methods afterwards, the experiments may be worth reciting.

Having put into a gun-barrel two ounces four pennyweights of red lead, I extracted from it twenty ounce-meaſures of dephlogiſticated air, receiving it in water; and the reſiduum, collected with all the care that I could apply, weighed 1 oz. 16 dwt. 18 gr.; ſo that twenty ounces of air ought to have weighed 7 dwt. 6 gr. which is beyond all proportion; ſo that this method muſt be very uncertain: beſides, no allowance was made, nor could well be made, for the fixed air which the red lead yielded, and which is the heavieſt ſpecies of air that we are yet acquainted with. At other times I have found that red-lead was changed into a real lead, when I was attempting the ſame thing in this way.

A ſecond attempt came a little nearer the truth. I weighed an ounce of red lead, moiſtened it with ſmoking ſpirit of nitre, and dried it, when it weighed 1 oz. 6 dwt. 12 gr. I then divided the whole quantity into two equal parts, and put one of them into a gun-barrel, in order to collect the air, and the other I put into a crucible, to be expoſed to the ſame degree of heat. The former yielded twenty-two ounce-meaſures of air, after the fixed air was pretty well waſhed out of it. It was about five times as good as common air. The latter had loſt nineteen grains in weight, being juſt ſo much leſs than half an ounce; ſo that the twenty-two ounce-meaſures of air ſhould have weighed nineteen grains, which is certainly a great deal too much: beſides, in this experiment, as in the former, no account could be taken of the fixed air.

Finding theſe methods to fail, I had recourſe to that which was uſed by Mr. Cavendiſh in weighing fixed and inflammable air, and which is more accurate than the method which I had uſed before (viz. filling a Florence-flaſk with the different kinds of air, and weighing them in it) becauſe, as the flaſk muſt be firſt filled with water, one cannot be ſure, though every poſſible precaution be taken, that the water has been equally drained from it after each experiment: otherwiſe there would be a conſiderable advantage in this method; becauſe the quantity of air may be accurately known. But though this cannot be done with preciſion in a bladder, as uſed by Mr. Cavendiſh, becauſe the degree of diſtenſion cannot be meaſured with much accuracy, yet this circumſtance is more than counterbalanced by being able to change the air with compreſſing the bladder, without wetting it.

I therefore took a glaſs-tube about nine inches long, and faſtening it to the neck of a bladder, which, with ſuch a degree of diſtenſion as I could give it, in the manner in which the experiment was made, contained fifty-five ounce-meaſures, or one pennyweight nine grains of common air. The tube was ſo faſtened, that I could take it out at pleaſure; and having the bladder thus prepared, I carefully compreſſed it, then filling it in part with that kind of air which I was about to weigh, I compreſſed it again, and then filled it intirely; ſo that I was pretty confident that the air within the bladder contained very little common, or any other kind of air. In this manner I proceeded to weigh *dephlogiſticated air*, and at the ſame time *nitrous air*, and *air phlogiſticated with iron filings and brimſtone*, which I take for granted is the ſame thing with air phlogiſticated by any other proceſs.

The following ſhort table exhibits the reſult of all theſe experiments at one view.

The bladder, filled with	dwts.	gr.
phlogiſticated air, weighed - - -	7	15
———————— nitrous air - - -	7	16
———————— common air - -	7	17
———————— dephlogiſticated air -	7	19

This reſult agrees ſufficiently well with my former obſervations, though they were not made with ſo much accuracy, viz. that both nitrous air, and air diminiſhed by phlogiſtic proceſſes, are rather lighter than common air; and it is conſonant to this, that, in the preſent experiment, dephlogiſticated air appears to be a little heavier than common air.

Comparing theſe obſervations with that of the extreme lightneſs of inflammable air, aſcertained by Mr. Cavendiſh, it ſhould ſeem that the leſs phlogiſton any kind of air contains the heavier it is, and the more phlogiſton it contains the lighter it is; though this is by no means the caſe with ſolid ſubſtances, and indeed it is rather unfavourable to this hypotheſis, that nitrous air ſhould not be lighter than dephlogiſticated air; for it ſhould ſeem, by its property of phlogiſticating common air, that it ſhould itſelf contain a greater proportion of phlogiſton. Alſo, in the above mentioned proceſſes for making air, the more phlogiſton there is in the ſubſtances moiſtened with ſpirit of nitre, the more certain it is that the produce will be nitrous air; as the leſs phlogiſton they contain, the more certain it is that the produce will be pure air. But I ſuſpect that there is a farther difference in the *mode* in which phlogiſton is combined with ſpirit of nitre, in the conſtitution of nitrous air.

In this experiment, the dephlogiſticated air was ſo pure, that one meaſure of it, and two of nitrous air, occupied the ſpace of $\frac{4}{5}$ of a meaſure. Had the air been more pure, it would, no doubt, have been ſpecifically heavier ſtill.

It ſhould be obſerved, that ſufficient time ought to be allowed to get dephlogiſticated air intirely free from fixed air before it is weighed;

and as this requires time, and perhaps may never be done completely, it may be ſuſpected that the additional weight of this kind of air is owing to a mixture of fixed air. But common air alſo contains a great proportion of fixed air, and the dephlogiſticated air, on which I made this experiment, had been produced, at leaſt the far greateſt part of it, and had been expoſed to water, ſome weeks. It is, however, ſufficiently evident, that dephlogiſticated air doth become better by ſtanding in water; owing, probably, to its depoſiting more fixed air in thoſe circumſtances.

Having at one time made a large reſervoir of dephlogiſticated air, for the purpoſe of experiments, I found that, in about ten days, from being 4 $\frac{1}{2}$, it had become 5 $\frac{1}{2}$ better than common air. Standing in pure water muſt be a ſurer method of getting the pureſt dephlogiſticated air, than *agitation* in water; for, though the latter method will enable the water to abſorb the fixed air faſter, and therefore a little agitation at the firſt will be very uſeful, in order to expedite the purification of it; yet, as I have found (vol. I. p. 158) that agitation in the pureſt water will, in time, injure common air; the ſame operation may be ſuppoſed to injure dephlogiſticated air alſo; and indeed I have already obſerved, that having agitated in water a quantity of dephlogiſticated air, a candle burned in it only as in common air, and not with that vivid flame with which it burns in this air when it is purer.

I have not made many experiments on the mixture of dephlogiſticated air with the other kinds of air, becauſe the analogy which it bears to common air is ſo great, that I think any perſon may know before-hand, what the reſult of ſuch experiments would be. It is pleaſing, however, to obſerve how readily and perfectly dephlogiſticated air mixes with phlogiſticated air, or air injured by reſpiration, putrefaction, &c. each tempering the other; ſo that the purity of the mixture may be accurately known from the quantity and quality of the two kinds of air before mixture. Thus, if one meaſure of perfectly noxious air be put to one meaſure of air that is exactly twice as good as common air, the mixture will be preciſely of the ſtandard of common air.

I obſerved alſo, in making this experiment, that after mixing one meaſure of each of theſe kinds of air, they made exactly two meaſures; ſo that there was neither any increaſe nor diminution of quantity in conſequence of the mixture, as is the effect of mixing nitrous air with either common or dephlogiſticated air.

It may hence be inferred, that a quantity of very pure air would agreeably qualify the noxious air of a room in which much company ſhould be confined, and which ſhould be ſo ſituated, that it could not be conveniently ventilated; ſo that from being offenſive and unwholeſome, it would almoſt inſtantly become ſweet and wholeſome. This air might be brought into the room in caſks; or a laboratory might be conſtructed for generating the air, and throwing it into the room as faſt as it ſhould be produced. This pure air would be ſufficiently cheap for the purpoſe of many aſſemblies, and a very little ingenuity would be ſufficient to reduce the ſcheme into practice.

I eaſily conjectured, that inflammable air would explode with more violence, and a louder report, by the help of dephlogiſticated than of common air; but the effect far exceeded my expectations, and it has never failed to ſurprize every perſon before whom I have made the experiment.

Inflammable air requires about two-thirds of common air to make it explode to the greateſt advantage; and if a phial, containing about an ounce-meaſure and half, be uſed for the experiment, the exploſion with common air will be ſo ſmall, as not to be heard farther than, perhaps, fifty or ſixty yards; but with little more than one-third of highly dephlogiſticated air, and the reſt inflammable air, in the ſame phial, the report will be almoſt as loud as that of a ſmall piſtol; being, to judge by the ear, not leſs than forty or fifty times as loud as with common air.

The orifice of the phial in which this experiment is made, ſhould not much exceed a quarter of an inch, and the phial ſhould be a very ſtrong one; otherwiſe it will certainly burſt with the exploſion. The repercuſſion is very conſiderable; and the heat produced by the exploſion very ſenſible to the hand that holds it. I have ſometimes amuſed myſelf with carrying in my pocket, phials thus charged with a mixture of dephlogiſticated and inflammable air, confined either with common corks or ground-ſtopples, and I have perceived no difference in the exploſion, after keeping them a long time, and carrying them to any diſtance.

The dipping of a lighted candle into a jar filled with dephlogiſticated air is alone a very beautiful experiment. The ſtrength and vivacity of the flame is ſtriking, and the heat produced by the flame, in theſe circumſtances is is alſo remarkably great. But this experiment is more pleaſing, when the air is only little more than twice as good as common air; for when it is highly dephlogiſticated, the candle burns with a crackling noiſe, as if it was full of ſome combuſtible matter.

It may be inferred, from the very great exploſions made in dephlogiſticated air, that, were it poſſible to fire gunpowder in it, leſs than a tenth part of the charge, in all caſes, would ſuffice; the force of an exploſion in this kind of air, far exceeding what might have been expected from the purity of it, as ſhewn in other kinds of trial. But I do not ſee how it is poſſible to make this application of it. I ſhould not, however, think it difficult to confine gunpowder in bladders, with the interſtices of the grains filled with this, inſtead of common air; and ſuch bladders of gunpowder might, perhaps, be uſed in mines, or for blowing up rocks, in digging for metals, &c.

Nothing, however, would be eaſier than to augment the force of fire to a prodigious degree, by blowing it with dephlogiſticated air inſtead of common air. This I have tried, in the preſence of my friend Mr. Magellan, by filling a bladder with it, and puffing it, through a ſmall glaſs-tube, upon a piece of lighted wood: but it would be very eaſy to ſupply a pair of bellows with it from a large reſervoir.

Poſſibly much greater things might be effected by chymiſts, in a variety of reſpects, with the prodigious heat which this air may be the means of affording them. I had no ſooner mentioned the diſcovery of this kind of air to my friend Mr. Michell, than this uſe of it occurred to him. He obſerved that poſſibly *platina* might be melted by means of it.

From the greater ſtrength and vivacity of the flame of a candle, in this pure air, it may be conjectured, that it might be peculiarly ſalutary to the lungs in certain morbid caſes, when the common air would not be ſufficient to carry off the phlogiſtic putrid effluvium faſt enough. But, perhaps, we may alſo infer from theſe experiments, that though pure dephlogiſticated air might be very uſeful as a *medicine*, it might not be ſo proper for us in the uſual healthy ſtate of the body: for, as a candle burns out much faſter in dephlogiſticated than in common air, ſo we might, as may be ſaid, *live out too faſt*, and the animal powers be too ſoon exhauſted in this pure kind of air. A moraliſt, at leaſt, may ſay, that the air which nature has provided for us is as good as we deſerve.

My reader will not wonder, that, after having aſcertained the ſuperior goodneſs of dephlogiſticated air by mice living in it, and the other teſts above mentioned, I ſhould have the curioſity to taſte it myſelf. I have gratified that curioſity, by breathing it, drawing it through a glaſs-ſyphon, and, by this means, I reduced a large jar full of it to the ſtandard of common air. The feeling of it to my lungs was not ſenſibly different from that of common air; but I fancied that my breaſt felt peculiarly light and eaſy for ſome time afterwards. Who can tell but that, in time, this pure air may become a faſhionable article in luxury. Hitherto only two mice and myſelf have had the privilege of breathing it.

Whether the air of the atmoſphere was, in remote times, or will be in future time, better or worſe than it is at preſent, is a curious ſpeculation; but I have no theory to enable me to throw any light upon it. Philoſophers, in future time, may eaſily determine, by comparing their obſervations with mine, whether the air in general preſerves the very ſame degree of purity, or whether it becomes more or leſs fit for reſpiration in a courſe of time; and alſo, whether the changes to which it may be ſubject are *equable*, or otherwiſe; and by this means may acquire *data*, by which to judge both of the paſt and future ſtate of the atmoſphere. But no obſervations of this kind having been made, in former times, all that any perſon could now advance on this ſubject would be little more than random conjecture. If we might be allowed to form any judgment from the length of human life in different ages, which ſeems to be the only *datum* that is left us for this purpoſe, we may conclude that, in general, the air of the atmoſphere has, for many ages, preſerved the ſame degree of purity. This *datum*, however, is by no means ſufficient for an accurate ſolution of the problem.

SECTION V.

Of Air procured from various Subſtances by Means of Heat only.

I have obſerved already, that, in my former experiments, I had not the uſe of a *burning lens* of any conſiderable force; and, for want of it, was obliged to leave many of the experiments extremely incomplete, and many things not even attempted. But having, ſoon after my late publication, provided myſelf with a lens of ſufficient force for the purpoſe, the firſt thing I did, when I began to reſume my experiments, was to make uſe of it, in order to ſatisfy myſelf what kind of air certain ſubſtances would yield by means of heat only, either in *vacuo*, or when confined by quickſilver; and it has been ſeen in the preceding ſections, that by purſuing this method, I was led to the diſcovery of many new and curious facts, of ſufficient importance to be conſidered ſeparately, and at large.

In this ſection, I propoſe to comprize the reſt of the obſervations that occurred to me in that courſe of experiments, intermixed with thoſe which I made in expelling air from the ſame ſubſtances, in a gun-barrel; having ſometimes made uſe of one of the methods, and ſometimes of the other, according to different circumſtances and views.

Theſe experiments were begun in June, 1774; and one of the firſt obſervations that I made was, that inflammable air may be procured from ſeveral metals by heat only, without any acid, which was not my opinion at the time of my former publications. I had rather thought that, becauſe, when the marine acid air had decompoſed ſubſtances containing phlogiſton (as brimſtone, charcoal, &c.) a quantity of inflammable air was produced, the acid air had contributed to its formation, and had entered into its conſtitution; and I had therefore inferred univerſally, that inflammable air conſiſts of acid air and phlogiſton. And becauſe inflammable air may be deprived of its inflammability, and from being highly noxious, become reſpirable by agitation in water, I had farther conjectured, that the air of the atmoſphere might conſiſt of the union of acid air and phlogiſton; and I do not ſee how any perſon could have avoided forming ſuch a conjecture from ſuch premiſes.

Nor indeed am I now abſolutely certain, that the concluſion was wrong; for the chymical principles are ſo altered, by combination, that many of them are known to exiſt where their preſence is leaſt of all ſuſpected: unleſs, however, there be an acid in metals, I do not ſee how my former opinion can be maintained, in conſiſtency with the facts that will appear in this ſection, viz. that inflammable air may be procured from ſeveral metals confined by quickſilver, with heat from a burning lens, without any ſolution of the metal in an acid.

It is evident, however, on the other hand, that inflammable air doth not conſiſt of pure phlogiſton; becauſe, as I have ſhewn, it may be intirely deprived of its inflammability; and though it be afterwards diminiſhed in bulk, yet a very great proportion of it remains; being then in the ſame ſtate with the air in which a candle has burned out, but ſufficiently pure for reſpiration. The queſtion is, *what is the baſis of inflammable air*, or what is the chymical principle to which the phlogiſton is united in its conſtitution. In this caſe it ſhould ſeem to be ſome new mode of combination with the earth of the metal. The facts, however, were as follows.

Having put a quantity of *iron-filings*, carefully ſorted with a magnet, into one of the glaſs-veſſels, fig. *a*, I filled the reſt of the veſſel with quickſilver; and placing it inverted in a baſon of quickſilver, I threw the focus of the lens upon the iron-filings, and preſently air was produced; which, being examined, appeared to be inflammable, though not very ſtrongly ſo. It reſembled inflammable air that had been waſhed in water till its inflammability was nearly gone. I alſo could not diſtinguiſh the colour of the flame, when I made the exploſion in the uſual manner, by the approach of a candle. After the operation, the iron from which the air had been extracted, had an exceedingly ſtrong ſmell, exactly like that of very ſtrong inflammable air procured from metals by acids.

In the ſame manner I got air from the *filings of watch-ſprings*, which are made of the beſt ſteel; and it was not to be diſtinguiſhed from the inflammable air of the laſt experiment. Theſe filings, as well as thoſe of iron, I had carefully ſorted with a magnet; ſo that I believe there was no foreign matter mixed with them. The greateſt care, however, is requiſite for this purpoſe, ſince, the leaſt bit of wood, or any vegetable or animal matter, hardly diſcocoverable by the eye, will yield more inflammable air, than a conſiderable quantity of iron-filings.

N. B. The ſpot on which the focus of the lens was thrown, was much blacker than any other part of the filings; and during the application of the heat, a quantity of the filings would ſometimes be diſperſed, as by an exploſion below the ſurface of them; owing, I ſuppoſe, to the ſudden generation of air from ſome of the filings that lay under the reſt, but where the heat could reach them.

Having thus got air from *iron*, I proceeded to make ſimilar experiments on other metals. But as all the other metals have more or leſs affinity with quickſilver, I was obliged to have recourſe to a *vacuum*; but being poſſeſſed of Mr. Smeaton's air-pump, I could depend upon the vacuum being very exact; ſo that very little common air could be mixed with the air produced. That the filings of the different metals might be perfectly unmixed, I procured new files, quite clean, and uſed one ſide of each for each of the metals.

With this apparatus, I threw the focus of my lens upon filings of *zinc*, and preſently got from them air which was very ſtrongly inflammable. Zinc is ſaid to contain more plogiſton than the other metals, and the difference between the inflammable air from zinc, and that which I got from iron, was very ſtriking.

From *braſs-duſt* I got inflammable air in conſiderable plenty, and alſo from tin; but this laſt was very ſlightly inflammable. I could not have perceived it to be ſo at all but by dipping a lighted candle into a veſſel full of it: whereas, in other caſes, I made the trials by preſenting the flame of a candle to the narrow mouth of a phial filled with the air. That braſs ſhould yield inflammable air, I attribute to the zinc, by the addition of which, copper is converted into braſs.

Thus all the metals that yield inflammable air, when diſſolved in acids, give inflammable air alſo by heat only. With other metals I had no ſucceſs.

Regulus of *Antimony*, heated *in vacuo*, ſmoked very much, and blackened all the inſide of my receiver; but the air that I got from it was very little indeed, and extinguiſhed a candle.

From *biſmuth*, and *nickel*, I got hardly any air at all; but in theſe experiments the heat was not advantageouſly applied, and the biſmuth ſoon melted into large lumps, on which my lens had no power.

I got no air from *lead* or *copper*. By throwing the focus of the lens upon the former, the receiver was filled with fumes; but the heat was by no means ſufficient for the experiment with copper.

In the account of my former experiments, I mentioned one that puzzled me and my friends exceedingly. It was that the air which I got from *chalk*, in a gun-barrel, was inflammable, and burned with a blue flame. I then conjectured, that this property came from the iron; and the experiments that I made with the burning lens have confirmed that conjecture. But why this inflammable air ſhould burn with a *blue flame*, I was long at a loſs to account for; ſince inflammable air from iron only, does not burn in that manner. At length it occurred to me, to try what would be the effect of burning inflammable air mixed with fixed air, procured from calcareous ſubſtances by acids; when I found it always burned with a blue flame. This fact I muſt have ſeen, perhaps, a hundred times, in a long courſe of experiments, on the mixture of fixed and inflammable air, made when I was very young in theſe inquiries, thinking that, together, they might be common air: but for want of attending to the *colour of the flame*, as not being the object I had then in view, I have been ſo much puzzled ſince. I am ſtill at a loſs to explain the reaſon of this effect of the mixture of theſe two kinds of air.

Fixed air is readily diſcharged from chalk by any acid; but it is a very ſmall quantity only, that mere *heat* will expel from it. However, from iron-filings and chalk, which I mixed together, in order to reſemble the circumſtances of the experiment with the chalk in the gun-barrel, I got air in great plenty; and this was exactly of the ſame kind with that which I had got from chalk in the gun-barrel. Very much of it was inflammable, and it burned with a blue flame. In a ſecond experiment of this kind, I had the ſame reſult.

No *calx* of any metal on which I made the experiment yielded inflammable air, but all of them fixed air, and generally in great plenty. *Ruſt of iron* gave a great deal of air, two-thirds of which was fixed air, and the reſt was not affected by nitrous air, and extinguiſhed a candle; ſo that the whole produce ſeemed to be fixed air, only with a larger reſiduum of that part which is not miſcible with water than uſual. At another time, however, I got from the ruſt of iron fixed air that was very pure, there being little of it that was not miſcible with water. It is poſſible, though I cannot pretend to recollect the circumſtances of the experiment, that I might uſe leſs heat in the latter caſe than in the former.

N. B. That part of the ruſt on which the focus of the lens fell, turned very black.

I obſerved in a former ſection, that both the *grey calx of lead*, and *litharge*, yielded fixed air, and that a great quantity of fixed air is contained in *red lead*, and in other preparations of that metal.

I got a little air by means of the burning lens in quickſilver, from *cinnabar prepared with antimony*; but not enough to form a judgment of the quality of it. From common *vermillion* I got more air, viz. about forty times its own bulk, and it was all fixed air, being readily abſorbed by water. This ſubſtance, like the ruſt of iron, turned black in the focus of the lens.

The *metallic ſalts*, if they gave any air at all, gave fixed air, which I find to be contained in moſt ſaline ſubſtances. I ſhall recite a few experiments of this kind, without any particular regard to the order of them.

White lead yielded air in great plenty, by the heat of the burning lens, and it was all pure fixed air.

I could get no air whatever from *ſugar of lead*, or from *nitre of lead*. The former melted into a liquid ſubſtance; the latter changed from white to a dull grey colour, and broke into powder, with a crackling noiſe.

All the kinds of *copperas* gave fixed air. I firſt tried common *green copperas* in quickſilver. It diſſolved into a great quantity of water, but the air produced from it was not $\frac{1}{20}$ of its bulk. Half of this air was readily abſorbed by water, and the remainder was too ſmall to be examined. I repeated the experiment on calcined copperas, both in a gun-barrel, and likewiſe in a tall glaſs-veſſel filled with ſand; but the produce, in all the caſes, was fixed air. Half an ounce of calcined copperas yielded near a pint of air.

When I had extracted air from the calx of green copperas in a glaſs-veſſel, I put the ſame materials into a gun-barrel; but ſtill I extracted nothing from them beſides fixed air, mixed with acid air, as appeared by the extremely ſmall bubbles to which the large ones were preſently reduced in paſſing through water.

When I made the experiment on *blue vitriol*, which conſiſts of oil of vitriol and copper, in quickſilver, the reſult was the ſame as with the green copperas, except that much leſs water was produced.

White vitriol, which conſiſts of oil of vitriol and zinc, gave ten times as much air as the other kinds. Half of it was abſorbed by water, and a candle burned in the remainder. When I extracted air from calcined white copperas, in a glaſs-veſſel, beſides fixed air, I got ſome that diminiſhed common air a little; but I conjecture that this nitrous property muſt have come from ſome other ſubſtance that was accidentally mixed with the vitriol.

Mercurial nitre gave a great quantity of air in quickſilver, and this was pure nitrous air; but poſſibly the nitrous acid being let looſe from this ſubſtance, had produced the nitrous air by diſſolving the quickſilver.

From *Roman vitriol revivified by flour*, which I had of Dr. Higgins, I got air, half of which was fixed, and the remainder was not diminiſhed by nitrous air.

All the air that I was ever able to get from *ſaline ſubſtances* was fixed air. I began with *alum*, and the firſt experiment that I made upon this ſubſtance was with the ſun-beams, in quickſilver; when I got from it a little air, which appeared to be fixed air, by extinguiſhing a candle, and by being readily abſorbed by water. I repeated the experiment with the ſame reſult. The quantity of air extracted from a piece of alum, was about one-third of its bulk; but I imagined that a little, though not much, more might have been extracted, by a longer continuance of the operation.

I obſerved, upon this occaſion, that I could calcine only a given quantity of alum in a given quantity of air; and that when this was ſaturated, I could only keep the alum in a fluid ſtate by heat. But it was eaſily calcined in vacuo; and as the receivers in which the calcination was made became very moiſt, it is pretty evident that this operation is performed by the mere expulſion of the water which enters into the compoſition of this ſalt; ſo that when the ſurrounding air can take no more water, that calcination can proceed no farther. I alſo obſerved, upon this occaſion, that when

I had calcined a quantity of alum in a given quantity of common air, the air was not diminiſhed, or in the ſmalleſt degree injured by the operation.

After this, I endeavoured to get air from *calcined alum*, with a burning lens; and I did get a little: but I made no other obſervation upon it, than that it was not diminiſhed by nitrous air. But when I put a quantity of calcined alum into a gun-barrel, I got from it a conſiderable quantity of air, part of which was fixed air, precipitating lime in lime-water, and the remainder did not differ from the reſiduum of fixed air, extinguiſhing a candle, and neither affecting common air, nor being affected by nitrous air.

From half an ounce of *vitriolated tartar*, in a gun-barrel, I got about 1 $\frac{1}{2}$ ounce-meaſure of air, which was chiefly fixed air. The laſt produce diminiſhed common air a little; but this I attribute to the gun-barrel, not having been perfectly cleaned from the materials uſed in a former experiment.

Borax was only melted by the burning lens; but *calcined borax* gave a little air, about its own bulk; and this air extinguiſhed a candle, and was not diminiſhed by nitrous air; ſo that it ſeems to be the ſame thing with the reſiduum of fixed air: and this is, in fact, much the ſame thing, if not quite the ſame thing, with common air phlogiſticated. I was induced to make this experiment, in conſequence of that which I had made on ſedative ſalt, which is made from borax, and from which, as I have obſerved, I had extracted air, about as good as common air; being in hopes that this experiment would throw ſome light upon the other; but I was diſappointed in that expectation.

Having thrown the focus of the burning lens upon a piece of *volatile ſal ammoniac*, in quickſilver, a great quantity of air was preſently expelled from it; but upon withdrawing the heat, a great part of it ſoon diſappeared, leaving the ſides of the veſſel covered with ſlender cryſtals, exactly like thoſe which are produced by a mixture of fixed air, and alkaline air. The remainder was imbibed by water, being, no doubt, fixed air.

Among other things, I threw the focus of the lens upon a piece of fine *white ſugar*, in quickſilver. It was readily melted, and converted into a brown ſubſtance, yielding about two-thirds of its bulk of air, one-third of which was readily abſorbed by water, and the remainder extinguiſhed a candle. I repeated the experiment with a browniſh powdered ſugar, with the ſame reſult, excepting that more air was generated from this than from the white ſugar, in proportion to their bulks.

From *common ſalt*, confined by quickſilver, I got no air at all.

There has been a good deal of difference of opinion among philoſophers, about the quality of the air that is really contained in chalk. Dr. Black's opinion is, that it is properly fixed air; whereas others have thought that the acid by which the air is diſlodged from the chalk, really enters into the air that is produced in the proceſs, and accordingly, that the fixed air produced by different acids, has different properties. An Italian philoſopher, who did me the honour to write to me upon the ſubject, informs me, that he has diſcovered that air produced from chalk by heat, is of a different nature from that which is got from it by acids, and particularly that the former will not make water acidulous. For my own part, I muſt acknowledge that I have not examined this ſubject thoroughly, and have been ſometimes inclining to one opinion, and ſometimes to another. Sometimes I have thought fixed air to be an *original acid*, and therefore one uniform invariable thing, from whatever ſubſtance, and in whatever manner procured. At other times I have been inclined to think, that its acidity is derived from ſome other acid, eſpecially the nitrous, for reaſons that may appear in a ſubſequent ſection.

At preſent, I cannot ſay that I am quite decided about this queſtion; but that I am much inclined to Dr. Black's opinion, and that all my experiments on chalk are in favour of it. For though I could get but very little air from pure chalk, either in quickſilver, or in vacuo, it was always fixed air, though the reſiduum was ſometimes more conſiderable than I have found it to be when the air was produced by the ſolution of chalk in an acid. Once, however, I got a ſmall quantity of very pure fixed air from chalk, by heat, in quickſilver, almoſt as much of it being abſorbed by water, as when chalk is made to give air by means of an acid.

It is remarkable, however, that heat is able to expel but very little air from chalk. I kept a very ſmall quantity of chalk in the focus of my burning lens, which I have obſerved to be twelve inches in diameter, and twenty inches focal diſtance, more than half an hour, when the ſun was near its greateſt altitude, on the

23d of July; but notwithſtanding this long expoſure to ſo intenſe a degree of heat, it ſeemed to give as much fixed air when thrown into a veſſel of water, acidulated with oil of vitriol, as an equal quantity of chalk which had not been expoſed to any heat at all. Of this, however, I only judged by the viſible efferveſcence, and did not make any attempt to meaſure the produce of air, in order to aſcertain the effect of theſe different circumſtances with accuracy. I have alſo kept chalk more than a quarter of an hour in the ſtrongeſt heat of a ſmith's forge, in a crucible, without making any ſenſible alteration in it. But I believe there may be great differences in the conſtitution of different ſpecimens of chalk in this reſpect.

When I put a quantity of chalk into a tall glaſs-veſſel, fig. *d*, and kept it in as ſtrong a ſand-heat as it would bear, without melting, I extracted from it about its own bulk of air; and examining the ſtate of it at ſmall intervals, I always found that it precipitated lime in lime-water, and that the reſiduum, not abſorbed by water, extinguiſhed a candle: and theſe ſeem to be the ſureſt teſts of genuine fixed air.

SECTION VII.

Of Air produced by the Solution of Vegetable Subſtances in Spirit of Nitre.

The experiments, of which an account will be given in this ſection, were occaſioned, in part, by a hint thrown out by Mr. Bewley, in his letter to me, printed in the *Appendix* of my former volume; but more immediately by an experiment which I had the pleaſure to ſee at Paris, in the laboratory of Mr. Lavoiſier, my excellent fellow-labourer in theſe inquiries, and to whom, in a variety of reſpects, the philoſophical part of the world has very great obligations.

Mr. Bewley ſays, that he had always taken it for granted, that the elaſtic fluid, generated in the preparation of nitrous ether, without diſtillation, was fixed air; but that, after ſeeing the firſt publication of my papers relating to air, he found, on examination, that it had the general properties of nitrous air.

At Mr. Lavoiſier's I ſaw, with great aſtoniſhment, the rapid production of, I believe, near two gallons of air, from a mixture of ſpirit of nitre and ſpirit of wine, heated with a pan of charcoal; and when that ingenious philoſopher drew this air out of the receiver with a pump, and applied the flame of a candle to the orifice of the tube through which it was conveyed into the open air, it burned with a blue flame; and working the pump pretty vigorouſly, he made the ſtreams of blue flame extend to a conſiderable diſtance. Being very much ſtruck with this experiment, I determined with myſelf to give particular attention to it, and purſue it after my return to England.

My firſt idea was, that this air was the ſame thing with the phlogiſticated nitrous air which I had procured by expoſing pieces of iron or liver of ſulphur to nitrous air, the phlogiſton of the ſpirit of wine being, as I ſuppoſed, diſengaged in this proceſs, and becoming incorporated with the nitrous acid, in the ſame manner as the phlogiſton that is diſengaged from the two other ſubſtances. Theſe kinds of air differed, however, in one reſpect, viz. that in Mr. Lavoiſier's experiment the flame was blue, whereas it had not been ſo in mine. But this ſeemed to be a circumſtance of no great importance. Indeed I cannot ſay, that, at preſent, my idea of the thing is materially different from what it was then; but I have ſince had an opportunity, by purſuing this experiment, of obſerving a much greater variety in the production of air by means of ſpirit of nitre, than I had any expectation of before.

In reality, the nitrous acid is of a moſt wonderful nature; the more I conſider it, the more it excites my admiration, and the more unfathomable the ſubject appears. I flatter myſelf that I have made conſiderable advances in the inveſtigation of it myſelf, and I ſtill propoſe to keep it in view; but I own I have very little expectation of ſeeing it thoroughly explained.

In general, it will be ſeen, in the courſe of theſe experiments, that if the ſubſtance with which the ſpirit of nitre is heated, whether it be fluid or ſolid, contain much phlogiſton, the air produced from it will be nitrous air, or poſſeſs the property of diminiſhing common air to a conſiderable degree; and, in almoſt all caſes, with a mixture of fixed air. If the ſubſtance be inflammable, the air will generally be ſuch as I ſaw at Mr. Lavoiſier's, burning with a blue flame. But this inflammability is of a very delicate kind, reſembling that of phlogiſticated nitrous air: for the air is eaſily deprived of it by waſhing in water.

A particular account of theſe experiments, though very remarkable in their nature, will, I foreſee, be thought tedious by ſome perſons; but the detail will be very uſeful to ſuch as ſhall chuſe to proſecute them; eſpecially on account of the precautions that I ſhall occaſionally give to prevent diſagreeable accidents from them. Every chymiſt knows how hazardous it is to mix ſpirit of nitre with inflammable matters; and I was not unapprized of it, having ſeen the effect in a courſe of chymical lectures many years ago. But, being obliged to make theſe mixtures in a very different manner, the effect could not be obviated without a variety of precautions, which experience only taught me.

Beginning with *ſpirit of wine*, in imitation of the experiment which I had ſeen at Mr. Lavoiſier's, I made the mixture with the ſpirit of nitre, in the manner directed in the proceſs for making nitrous ether; putting about one-third of ſpirit of nitre, to two-thirds of ſpirit of wine, in ſuch phial as *e*, vol. I. plate I.; mixing them very gradually. Heating this mixture with the flame of a candle, I received the air in water; and when I had procured a conſiderable quantity of it, I examined it, and found it to burn with a gentle blue, or greeniſh flame, nearly the ſame, as well as I could recollect, with that which I had ſeen at Mr. Lavoiſier's; ſo that I had no doubt but that my proceſs, though ſomewhat different from his, had anſwered perfectly well.

Conſidering this flame with attention, I thought it very much reſembled that which is produced by a mixture of about one-third inflammable air, and two-thirds nitrous air; and concluded, that it was probably compoſed of them both; the nitrous acid forming nitrous air, by ſeizing upon the phlogiſton of the ſpirit of wine; and there being a redundancy of inflammable matter, ſufficient to render the air partially inflammable.

In the directions to make nitrous ether, I was cautioned to pour the ſpirit of nitre upon the ſpirit of wine, and by no means to pour the ſpirit of wine upon the ſpirit of nitre. But though this method of mixing theſe liquids may not anſwer the purpoſe of making nitrous ether, it anſwered very well for the production of air, and was a very uſeful variety in the proceſs. It is neceſſary, however, that the unexperienced operator ſhould be upon his guard in theſe experiments.

The ſpirit of nitre ſhould be much diluted, and the quantity of any liquid inflammable matter ſhould be very ſmall, juſt ſufficient to cover the ſurface of it: otherwiſe, though the mixture may exhibit no alarming appearance at firſt, it will, in a little time, become very black, beginning at the ſurface; the phial will then be filled with red fumes, the air will be generated in a prodigious torrent, and, unleſs the tube through which it is tranſmitted be ſufficiently wide, and the veſſel in which the mixture is made be very ſtrong, the whole will be exploded with great violence. Of this I have ſeen but too many inſtances; and ſometimes when I had thought that my experience had taught me ſufficient precaution. Beſides, all oily matters become extremely viſcid, by mixing with ſpirit of nitre; and this viſcid matter getting into the tube, ſtops it up, and much increaſes the hazard of an exploſion. But to recur to the experiments.

Having poured a very little *ſpirit of wine* upon a quantity of diluted ſpirit of nitre in a glaſs-phial, with a ground ſtopple and tube, a great quantity of air was preſently produced. When a candle was dipped into this air, it was extinguiſhed; but in going out was ſurrounded with a ſlight blue or green flame, but hardly more than is perceived in nitrous air. Almoſt one-half of this produce of air was readily abſorbed by water, and precipitated lime in lime-water; and I doubt not but that, in the ſubſequent experiments, as well as in this, a great proportion of the air produced in this manner was fixed air. The remainder was nitrous, almoſt as ſtrong as any.

Upon air produced in this manner from *oil of turpentine*, I happened to make a few more experiments, ſome of which are not a little remarkable. When I uſed the ſtrongeſt ſpirit of nitre in this proceſs, it was very difficult to get much air, on account of the ſuddenneſs of the effervescence; but a great quantity of air is eaſily produced by diluting the ſmoking ſpirit of nitre with an equal quantity of water. At one time, however, when I had heated this mixture pretty much, and it had yielded a great deal of air, though I withdrew the candle, the air continued to be produced faſter and faſter for about a minute. It then came quite in a torrent; all the oil of turpentine was thrown out of the phial, and the ſpirit of nitre only left in it. This is likewiſe the caſe with other ſimilar mixtures; ſo that when it is neceſſary to apply heat, it ſhould be done very gradually and cautiouſly, and the air ſhould never be generated very faſt, unleſs the purpoſe of the experiment require it, and the operator be upon his guard accordingly.

When I received this air in water, it extinguiſhed a candle, and did not diminiſh common air. When received in quickſilver, it ſtill extinguiſhed a candle; but as it went out the third or fourth time, it was ſurrounded with a bluiſh flame, as large as that of the candle. And happening, at one time, to apply more heat than I intended when the air was received in water (and in conſequence of it, the air was produced very ſuddenly) I examined it immediately, and a candle burned in it with an enlarged flame, though not remarkably ſo. It ſhows, however, that in this proceſs alſo, as well as in the proceſs for making phlogiſticated nitrous air, the property of its admitting a candle to burn with an enlarged flame depends, in a great meaſure, upon the *time* at which the experiment is tried after the air is produced, and upon other delicate circumſtances.

A quantity of this air, received in water, was about half-abſorbed in one night. By agitation it appeared to be abſorbed not ſo readily as fixed air, nor with ſo much difficulty as nitrous air, but in a medium between both. When this air was reduced to about one-eighth of its original bulk, it was diminiſhed by nitrous air. But this is the caſe with all the kinds of air that will bear the experiment, and even with nitrous air itſelf, as I have obſerved in my former publication.

At the time that I made the preceding experiments with oil of turpentine, I had no lime-water at hand; and therefore only judged that part of the produce was fixed air, by the manner in which it was abſorbed by water. But, leſs certain as this teſt is, a perſon much uſed to experiments of this kind, will be able to apply it with ſufficient certainty in moſt caſes. However, repeating this experiment, when I had procured the glaſs-phials with ground-ſtopples and tubes, I found that the greateſt part of this air was unqueſtionably fixed air, precipitating lime in lime-water, as much as any fixed air whatever, and that the remainder was ſtrongly nitrous. Attempting at this time alſo, to receive the air in quickſilver, a good deal of the vapour of the ſpirit of nitre came over; and, diſſolving the quickſilver, made the produce of air almoſt wholly nitrous.

I obſerved, at one time, when I had produced this air in a phial with a ground-ſtopple, that after the firſt part of the proceſs, in which no heat was applied, the water ruſhed back into the phial. Upon this I applied the flame of a candle to the diluted mixture, and getting a ſecond produce of air, examined them both ſeparately. Both of them contained a great proportion of fixed air, precipitating lime in lime-water very much; and when the fixed air was waſhed out of them, they both diminiſhed common air, but the latter more than the former. Two meaſures of common air, and one of this, occupied the ſpace of little more than two meaſures.

In order to judge how far an *acid* prevailed in this air from ſpirit of nitre, and oil of turpentine, I put alkaline air to it; when inſtantly a white cloud was produced, which roſe to the top of the veſſel; but it was by no means ſo denſe as that which is produced by mixing alkaline air with any of the acid airs; nor did the whole quantity of air diſappear, but only half of it. However, all the inſide of the tube was covered with a ſaline ſubſtance, which I did not examine, but ſuppoſed it to have been the *nitrous ammoniac*. Having the curioſity to dip the flame of a candle, which happened to be at hand, into the air that remained of this mixture, it appeared to be ſo far inflammable, as even to make a conſiderable exploſion; but not quite ſo great a one as I have obſerved to have been made by a quantity of phlogiſticated nitrous air, vol. I. p. 217.

Repeating this experiment ſome time afterwards, about one-fourth of the mixture of this air, and alkaline air, diſappeared upon their being put together. Half of the remainder was abſorbed by water; and in this ſecond remainder, which, by its redneſs, on being expoſed to common air, appeared to be conſiderably nitrous: a candle burned with a beautifully enlarged flame.

In theſe caſes the alkaline air muſt have ſupplied the phlogiſton, which the iron and liver of ſulphur had before ſupplied to nitrous air; in conſequence of which it admitted a candle to burn in it in the ſame manner; for neither of the component parts of this air, viz. the fixed or the nitrous, are either ſeparately, or together, inflammable. It is ſomething remarkable, however, that when I mixed equal quantities of nitrous and alkaline air, and examined the mixture immediately, the nitrous air ſeemed not to have been at all affected by the alkaline air. It was not in the ſmalleſt degree inflammable. I had imagined that alkaline air might, in this manner alſo, have phlogiſticated the nitrous air; but it ſeems that when it is ſo applied, it has no ſuch effect.

Air produced from all the *eſſential oils* by ſpirit of nitre, has, I believe, the ſame properties as that which is produced from oil of turpentine. I tried another, but I forget which, in a phial with a ground-ſtopple, and the air produced from it precipitated lime in lime-water, extinguiſhed a candle, and diminiſhed common air a little.

Ether, both vitriolic and nitrous, heated in ſpirit of nitre, yields the ſame kind of air as the eſſential oils, or ſpirit of wine, viz. partly fixed air, and partly phlogiſticated nitrous air. Equal caution is alſo neceſſary in conducting this proceſs; for the phenomena attending it are the ſame that I deſcribed in the beginning of this ſection, and in the higheſt degree. I would therefore recommend the uſing of a very ſmall quantity of the ether, and putting it upon the ſpirit of nitre.

At firſt, however, in imitation of the proceſs for making nitrous ether, I poured the ſpirit of nitre upon the ether, as I had done at firſt alſo with ſpirit of wine; and, heating the mixture, received the air, which it yielded in great plenty, in quickſilver. This air made no cloud with the mixture of alkaline air; it burned exactly like the vapour of ether itſelf; and when part of the mixture had boiled over, it quickly abſorbed the air that had been generated.

Seeing ſufficient reaſon to diſapprove of this proceſs, I had recourſe to the other, and found that when I uſed a very diluted ſpirit of nitre, and but little ether, the experiment was much more manageable, and the air was produced in ſufficient plenty. This air was readily abſorbed by water; and upon putting alkaline air to it, a very ſlight cloud roſe to the top of the veſſel; but there was no ſenſible diminution of the quantity of air occaſioned by it. When a candle was dipped into this air, it was extinguiſhed many times, but always with a beautiful bluiſh flame, much larger than the natural flame of the candle. Towards the cloſe of the experiment, the air in the inſide of the veſſel became red; a certain ſign of its being conſiderably nitrous. On repeating this experiment, when I had procured the phials with the ground-ſtopples and tubes, I had the moſt ſatisfactory proof, that part of this produce of air was fixed air, by its precipitating lime in lime-water; and that the remainder was nitrous, almoſt as ſtrong as any, by its power of diminiſhing common air.

The reſult of the experiment with *nitrous ether* was, in all reſpects, the very ſame as that of this with vitriolic ether. I made the experiment, becauſe it might have been expected that there would have been ſome difference in the reſult, as the nitrous ether is the produce of ſpirit of nitre, with which it was now mixed.

Spirit of nitre, heated with *olive-oil*, yields the ſame kind of air with that which is produced from eſſential oils, &c. but the proceſs is exceedingly troubleſome, owing to the tenacity of the oil; and it is not much more manageable, when but a very little of the oil is put to a large quantity of the diluted ſpirit of nitre. The air which I got in this manner precipitated lime in lime-water.

With very great difficulty I got, in a phial with a ground-ſtopple, a very ſmall quantity of air from ſpirit of nitre and *tallow*, the water ruſhing into the veſſel after every guſh of air. It precipitated lime in lime-water.

The reſult of the experiment with *bees-wax*, was the very ſame with that with tallow. Putting a ſmall piece of bees-wax upon a quantity of pretty ſtrong ſpirit of nitre, I got air which made lime-water turbid; but not enough to aſcertain its other properties. This proceſs was equally difficult with the preceding, on account of the water ruſhing into the phial after every guſh of air.

I had the curioſity to endeavour to procure air from ſome of the *gums*, &c. by this proceſs, and found the reſult to be, in the main, the ſame with that of the preceding experiments.

Gum-arabic eaſily diſſolves in the nitrous acid; and as it diſſolves, a great quantity of air is produced, making a beautiful appearance; but when the acid is nearly ſaturated, it becomes viſcid, and the veſſel gets full of froth. Part of this air was fixed, precipitating lime in lime-water, and being readily abſorbed by water. The remainder was nitrous, almoſt as ſtrong as any.

The reſult was the ſame with *gum-copal*, excepting that this ſubſtance did not ſink in the ſpirit of nitre, as the gum-arabic had done.

Camphor, with diluted ſpirit of nitre, yielded very ſtrong nitrous air; but required a conſiderable degree of heat. A good deal of the camphor, which had been fluid, and had ſwum on the ſurface of the ſpirit of nitre, came over, and reſumed its natural appearance in water. I did not try whether any part of this produce was fixed air.

I got ſome air by ſpirit of nitre from *amber*, which precipitated lime in lime-water; but the quantity was too ſmall to be examined any farther. Afterwards I got a larger quantity from a greater number of ſmall pieces of amber, heated in a weak ſpirit of nitre, contained in a phial with a ground-ſtopple. About one-third of this produce was fixed air, precipitating lime in lime-water, and being readily abſorbed by water. In the remainder a candle burned with an enlarged greeniſh flame. It alſo diminiſhed common air; ſo that two meaſures of common air, and one of this, occupied the ſpace of 2 $\frac{1}{4}$ meaſures.

N. B. Moſt of the pieces of amber uſed in this experiment were turned black quite through, the reſt continuing of their natural colour.

It happened, in the courſe of theſe experiments, that a bit of *ſealing-wax* got into the phial, and I obſerved air to iſſue from it very copiouſly. Upon this, I put a piece of ſealing-wax into the phial, with ſpirit of nitre, and received the air at different times. That which came over firſt was, in the higheſt degree, nitrous; but when, with the application of more heat, I cauſed a copious production of a very turbid kind of air (which however, preſently became tranſparent) it hardly affected common air at all. It was then pretty readily abſorbed by water; and though at firſt it extinguiſhed a candle, yet when it had been waſhed in water, a candle burned in it with a blue flame. Indeed when the candle was extinguiſhed in it, it went out with that kind of blue flame. The courſe of this experiment will be found to be analogous to that with other *hard ſubſtances* containing phlogiſton, which I ſhall now recite, though many of them were made before this.

Having found that *charcoal* would diſſolve in oil of vitriol, and thereby yield a vitriolic acid air, I had the curioſity to try what would be the effect of an attempt to diſſolve this ſubſtance in ſpirit of nitre. This was when I had made but little progreſs in the preceding experiments with oily and gummy ſubſtances, and I had no expectation of the reſult. I began with taking the produce in quickſilver, as I had done with that from the vitriolic acid; but all that came over in this manner, was the nitrous acid vapour, which, ſeizing upon the quickſilver, produced nitrous air.

After this, I received the produce in water, and found it to be genuine nitrous air, almoſt as ſtrong as any that is produced from metals. At that time I was much ſurprized at this reſult,

ſult, having imagined that nitrous air could not be procured but by the ſolution of metals in ſpirit of nitre; and I conſidered this as another property in which metals and charcoal reſemble each other; beſides thoſe which I had noted before, and an account of which may be ſeen in a paper formerly printed in the Philoſophical Tranſactions, and which I ſhall inſert in this volume. But preſently after this I got nitrous air equally ſtrong from other hard ſubſtances, ſuch as *dry wood* of various kinds, &c.; but in theſe proceſſes, the quality of the air differs exceedingly, according to the *degree of heat* applied, and other circumſtances: and I think the ſubject deſerves a farther inveſtigation. To promote this, I ſhall recite the principal facts of this kind that have occurred to my obſervation.

Having poured about a quarter of an ounce-meaſure of ſmoking ſpirit of nitre, mixed with an equal quantity of water, upon ſome *pounded charcoal*, and having applied to it the flame of a candle, I collected a large jar full of air, in all twenty-eight ounce-meaſures. When about half of this quantity of air was produced, it was impoſſible to apply any more heat, but the ſpirit of nitre would come over; which it did, tinged with a deep black. When all the liquor was come over, ſtill one-fourth part of the air was produced with the application of a ſtrong heat. The air of this whole produce, which was not taken at different times, was ſtrongly nitrous. Two meaſures of common air, and one of this, occupied the ſpace of no more than two meaſures.

It was my ſeeing this air produced in different circumſtances, viz. before any of the acid came over, and afterwards, that ſuggeſted to me the importance of taking the air at different times, according to the change of circumſtances in the production of it; a hint which I purſued to very great advantage afterwards, as the reader has already ſeen, and will ſee farther, in the courſe of my experiments.

Repeating the experiment with this view, I examined the firſt produce of air, which came over, while the heat was very moderate, and found it to be very ſtrong nitrous air, almoſt as ſtrong as that which is procured from metals. Towards the laſt I increaſed the heat, and by that means produced a very turbid air, of which I collected a prodigious quantity. Sometimes, however, the air would be quite tranſparent, and then turbid again, ſeveral times. I endeavoured to take the turbid air and the tranſparent ſeparately, and I ſucceeded pretty well; but I found them both to be of the ſame quality, extinguiſhing a candle, and diminiſhing common air but very little; two meaſures of common air, and one of this, occupying the ſpace of little leſs than three meaſures.

At this time I made uſe of the phials repreſented fig. *a*, with common corks; and obſerving that the corks were always much corroded in theſe experiments, I thought it would be proper to aſcertain the effect of the ſpirit of nitre on the *cork*, in order to make proper allowance for this circumſtance in future experiments. I therefore poured a quantity of ſpirit of nitre upon ſome pieces of cork, and treating it in the manner above mentioned, I found the produce of air to correſpond very exactly with that which I had got from the charcoal. With a moderate degree of heat the air was ſtrongly nitrous; and with a great heat the air was turbid, and much leſs nitrous. I was not a little ſurprized to find that nitrous air was produced from cork, as it intirely overturned my ſyſtem of the production of this air, depending upon that property of the charcoal by which it reſembles metals. However, I preſently found, that genuine nitrous air was produced from a variety of other *hard ſubſtances*; for at that time I had not diſcovered that it was produced from any liquid ones.

The correſpondence of an experiment which I made with old dry oak with that which I made with charcoal is ſtriking enough; and one of them may a little illuſtrate the other.

I put about half an ounce meaſure of the raſpings of *old dry oak* into one of the phials above mentioned, fig. *a*, and poured upon them as much ſpirit of nitre, half-diluted with water, as made them thoroughly moiſt. Air was inſtantly produced, without the application of any heat. This air I received, togegether with a little that was produced by holding the flame of a candle, at the diſtance of about a quarter of an inch, from the ſide of the phial. I then placed the candle nearer, and received the air at five different times; the laſt but one being produced when the flame touched the ſide of the phial, and the laſt, when it was placed cloſe under it, and after all the moiſture ſeemed to be expelled from the phial The firſt produce was of the nature of nitrous air, the two next much more ſo, almoſt as ſtrong as any; but the two laſt were hardly nitrous at all. A candle went out in this air, burning with a bluiſh flame, as if it had been in part a mixture of inflammable, nitrous, and fixed air. That part of this produce was fixed air, was evident, by its being readily abſorbed by water; but I did not apply to it the teſt of lime-water.

Seeing this aſtoniſhing difference in the produce of air by ſpirit of nitre from different ſubſtances, and even from the ſame ſubſtance in different circumſtances, I thought that it might be poſſible, by this means, to diſtinguiſh thoſe ſubſtances that are *nutritious* from thoſe that are not; and, in my imagination, I had thought it poſſible to aſcertain the *quantity of nutriment* that different ſubſtances would yield by the quality and quantity of the air produced from them; but the experiments by no means anſwered ſuch fond expectations. I found, however, what I did not expect, viz. a moſt remarkable difference between the air produced from *animal ſubſtances* of ſeveral kinds, and from *vegetables*; for, in general, the former had little of the nitrous property; but the latter, though nutritious, yielded the ſame kind of air with that which I had got from wood or charcoal. The facts ſurprized me very much and I can give the reader no clue to lead him through the labyrinth.

The vegetable ſubſtances which I tried were *wheat-flour*, *barley*, and *malt*, all of which yielded nitrous air in the firſt part of the produce, and air of the ſame quality with the laſt produce from charcoal, if the proceſs was continued a long time, and with a ſtrong heat. I had once ſuſpected that the nitrous quality might have come from the cork with which the phial was cloſed; but I was ſatisfied that it came from the ſubſtance within the phial, when, inſtead of a phial cloſed with a cork as before, I uſed one of thoſe repreſented fig. *b*, which I have obſerved to have been contrived by Mr. Vaughan. Having put the barley and ſpirit of nitre into this veſſel, I heated it in a veſſel full of water, placed on the fire, covering the phial with a glaſs jar filled with water, in order to receive the air. The air procured in this manner was ſtill ſtrongly nitrous, though it could come from nothing but the ſpirit of nitre and barley.

As I attended to a few collateral circumſtances in the experiment with the *malt*, it may be worth while to recite the particulars. Having juſt covered one pennyweight of malt with diluted ſpirit of nitre, I made it boil, and procured from it two jars full of air, each containing near thirty ounce-meaſures, and I might have collected more. That which came firſt, and which was tranſparent, diminiſhed common air almoſt as much as the ſtrongeſt nitrous air. The air which came laſt, and which was turbid, hardly diminiſhed common air at all, and it was readily abſorbed by water. Before it was agitated in water, it extinguiſhed a candle; but afterwards, when it was reduced to about one-fourth of its original quantity, a candle burned in it with a lambent blue flame.

N. B. Towards the cloſe of this proceſs, part of the contents of the phial were reduced to a coal.

SECTION VIII.

Of Air procured by the Solution of ANIMAL SUBSTANCES *in Spirit of Nitre.*

I profeſs not to be able to aſſign any reaſon for the difference in the produce of air from *animal* and *vegetable* ſubſtances; but the experiments, of which an account will be given in this ſection, compared with thoſe recited in the laſt, will prove, that, in general, there is a very conſiderable one.

It has been ſeen that vegetable ſubſtances, diſſolved in ſpirit of nitre, beſides fixed air, yielded nitrous air, and frequently as ſtrong as that which is procured by the ſolution of metals in the ſame acid; and this is the caſe whether the ſpirit of nitre be much concentrated, or much diluted. On the contrary, animal ſubſtances, in general, treated in the ſame manner, yield about the ſame proportion of fixed air; but the reſiduum is either not at all, or in a very ſlight degree, nitrous (except in ſome caſes where the ſpirit of nitre is very ſtrong) but is a kind of air which, neither affecting common air, nor being affected by nitrous air, but ſimply extinguiſhing a candle, may be termed *phlogiſticated air.* Towards the end of a proceſs, indeed, when, by means of a ſtrong heat, the produce of air is very rapid, and the air full of clouds, it is, like air produced from vegetable ſubſtances in the ſame circumſtances, ſlightly inflammable, burning with a lambent, greeniſh, or bluiſh flame.

As there is a conſiderable variety in the reſult of theſe proceſſes, ariſing from ſeveral circumſtances, the influence of which may not be apprehended, I have been careful to note every thing relating to them, that appeared to me at the time to be of any importance. But, notwithſtanding this, it is very poſſible I may have made omiſſions, of the effect of which I was not apprized; and therefore thoſe who ſhall endeavour to repeat the experiments after me may not find preciſely the ſame reſults that I have reported. This will often be the caſe in experimental inquiries ſo new as theſe; and as no human care has yet been ſufficient to prevent this inconvenience, it is the part of human candour to make proper allowance for it.

I cannot help flattering myſelf, however, that theſe experiments, properly purſued, may be a means of throwing light upon the two great natural proceſſes of *vegetation* and *animalization*;

lization; as they exhibit a new and ſtriking difference between ſubſtances formed by them. On this account I would willingly recommend them to the particular attention of chymiſts and phyſicians. The experiments themſelves, nearly in the order in which they were made, are as follows.

I put equal quantities of ſpirit of nitre and water upon ſome pieces of *beef*, dried till they were perfectly hard, but without being burned, and took the firſt produce of the air, which was generated without the application of heat, and was very conſiderable; and afterwards that which came over when the flame of a candle was placed within about a quarter of an inch from the phial; but neither of them ſenſibly affected common air. They were both pretty readily abſorbed by water, and extinguiſhed a candle. I had expected that this air, like that from dry wood, would have been nitrous air.

This experiment being made with the fleſhy part of a *muſcle*, I next took a *tendon* from a neck of veal, imagining, from its firmer texture, that the air produced from it might approach nearer to that from wood; but the air that came from it neither diminiſhed common air, nor was diminiſhed by nitrous air, nor was it readily abſorbed by water, and a candle went out in it. It ſeemed, upon the whole, to be much the ſame thing with phlogiſticated common air.

I thought there might be ſome difference in this reſpect, between air produced from the *white*, and from the *brown* fleſh of animals; but I made the experiment with the breaſt and the leg of a *turkey*, without finding any. That which was produced from theſe ſubſtances exactly reſembled the air that I had got from the tendon of the calf; except that it was more readily imbibed by water. I agitated a quantity of it in water five minutes, when one-fourth of it was abſorbed, but the remainder ſtill extinguiſhed a candle, and did not differ from what it was before, except that it was now diminiſhed by nitrous air, like all other kinds of air agitated in water. When all the fleſh was diſſolved, air was ſtill produced in great plenty, upon the application of the flame of a candle. The air produced in this manner was very turbid at firſt; but the quality of it was not ſenſibly different from that which came firſt, and which was tranſparent.

I repeated this experiment with the ſame event, obſerving that the turbidneſs of the air depended upon the *degree of heat* with which

it

it was produced; for, after producing a large quantity of turbid air, I leſſened the heat, and preſently the air was tranſparent as at firſt, and upon increaſing the heat, the air was turbid again.

Having found no air of the nitrous kind from the fleſh of an animal of the *quadruped* ſpecies, or of a fowl, I was willing to try what would be the produce from the fleſh of *fiſhes*, *inſects*, and *exanguious* animals.

From the fleſh of *ſalmon*, made thoroughly dry, and then diſſolved in ſpirit of nitre, I got a great quantity of air, at firſt without heat, till the whole was nearly diſſolved; when about a quarter of an ounce meaſure of this ſolution ſtill yielded more than a quart of air. At the laſt this liquor, which had been pretty clear, became ſuddenly opake; and in this ſtate it yielded air the moſt plentifully, and continued to do ſo till, all the moiſture being evaporated, it became a dry coal. While it continued clear, a ſtrong heat, occaſioned by applying the flame of a candle cloſe to the phial, would immediately make the air turbid, eſpecially toward the end of the proceſs, juſt before the liquor became opake. At this time, however, the air in the inſide of the phial had nothing of that appearance, nothing being ſeen in it but the red fumes of the ſpirit of nitre; but when the liquor became opake, it was filled with very denſe white fumes.

The air, in all the ſtages of this experiment, was in part fixed, precipitating lime in lime-water. In the middle of the proceſs the reſiduum was nitrous; but only in a ſlight degree. Towards the concluſion it had no ſenſible effect on common air; and at laſt it burned with a blue lambent flame, which continued a conſiderable time after I had withdrawn the candle by which it had been ſet on fire. In the air that came juſt before the laſt, a candle barely went out, ſurrounded by a ſlight flame of that colour.

Repeating the experiment, I found nothing nitrous, either in the firſt produce of air, before the fleſh was diſſolved, or afterwards; and at this time I was particularly careful not to uſe any of the fleſh that was turned black, or very brown, in drying; having ſome ſuſpicion that the nitrous property of the air in the preceding experiment came from ſuch parts of the fleſh, being then a kind of charcoal.

The fleſh of ſalmon having a peculiar colour and flavour, I thought it would not be amiſs to repeat the experiment with ſome other kind of fiſh, the fleſh of which was white and taſteleſs. I therefore took the *fleſh of perch*, and diſſolving it in ſpirit of nitre, I procured a large quantity of air, no part of which was nitrous; but a conſiderable part of it was fixed, precipitating lime in lime-water. The greateſt part of this air was produced after all the fleſh was diſſolved; and at the laſt, when I increaſed the heat, the air was turbid; but it did not ſenſibly differ from that which was produced at firſt, except that a candle went out in it with the flame ſlightly tinged with green.

A large *worm*, treated in the ſame manner, yielded air that was in part fixed, making lime-water turbid. The reſiduum extinguiſhed a candle, and was, in a ſmall degree, nitrous; owing, perhaps, to ſomething on its ſtomach; for I had only preſſed out the contents with my finger.

Air produced from a number of *waſps*, diſſolved in ſpirit of nitre, was partly fixed, and the reſiduum ſo far nitrous, that two meaſures of common air, and one of this, occupied the ſpace of 2 $\frac{1}{2}$ meaſures. When the flame of a candle was dipped into it, it burned with a greeniſh lambent flame.

I had next the curioſity to try what kind of air might be procured from the inſenſible *excreſcences of animal bodies*, as *horn*, *hair*, *feathers*, &c. which are protruded from the body, and ſeem, at firſt ſight, to be in a kind of intermediate ſtate between vegetable and animal ſubſtances; but they appeared to be more of an animal than of a vegetable nature, i. e. judging by the air which I had hitherto found thoſe ſubſtances to give.

With ſpirit of nitre and *hair* I got a quantity of air, part of which was fixed, precipitating lime in lime-water, and the remainder, not abſorbed by water, which was about two-thirds of the whole, was in a ſmall degree nitrous.

From a *crow-quill* I got air of the ſame quality with that from the hair in the preceding experiment. This quill was black; and thinking it poſſible (as the hair I had made uſe of was alſo in part black) that the nitrous property of the air might come from the phlogiſton which produced that colour, I repeated the experiment with a *white feather*; but the reſult was the ſame: or, rather, the air in this caſe, was more nitrous than in the former. Two meaſures of common air, and one of this, occupied the ſpace of 2 $\frac{1}{2}$ meaſures. Had I uſed a much diluted ſpirit of nitre, it will appear probable, from the experiments recited at the cloſe of this ſection, that the produce would have been leſs nitrous.

Air was eaſily procured by diſſolving *horn* in ſpirit of nitre. Part of it was fixed air, precipitating lime in lime-water; but a very great proportion of it was not abſorbed by water. In this reſiduum there was nothing ſenſibly nitrous. That which came firſt extinguiſhed a candle, without any particular appearance; but in that which came laſt, it burned with a beautifully blue lambent flame.

I had thought that, poſſibly, the *inſide of an oyſter-ſhell*, or *mother of pearl*, might, together with fixed air, yield a quantity of ſuch phlogiſticated air as had been produced in the preceding experiments; but when they were diſſolved in ſpirit of nitre, they each of them gave very pure fixed air, without any greater reſiduum than is found in the ſolution of chalk in oil of vitriol.

Pieces of *ivory* diſſolved in a very beautiful manner, in hot ſpirit of nitre, and yielded a great quantity of air, which, in every ſtage of the proceſs, precipitated lime in lime-water. The reſiduum was not nitrous, and extinguiſhed a candle, without any particular colour of the flame.

To try the difference between the ſame ſubſtance, in a natural ſtate, and after it was reduced to a coal by fire, I diſſolved ſome *charcoal of ivory* in ſpirit of nitre, and found that it yielded plenty of air, the greateſt part of which was fixed, and the reſiduum was conſiderably nitrous. When the air was produced very faſt, the inſide of the phial was filled with a white fume. This ivory had been kept in a red heat, covered with ſand, about an hour.

Eggs do not rank with the ſubſtances above mentioned; but being the produce of an animal, and yet no proper part of one, I ſhall recite the experiments I made upon them in this place. Both the *white* and the *yolk* of an egg, which I tried ſeparately, yielded a conſiderable quantity af air, when diſſolved in ſpirit of nitre, and the difference between them was not ſenſible. In both caſes part of the air was fixed, precipitating lime in lime-water, and the reſiduum was ſo far nitrous, that two meaſures of common air, and one of this, occupied the ſpace of 2 $\frac{1}{2}$ meaſures.

It occurred to me, that, poſſibly, other parts of the animal, and the different animal *ſecretions*, might yield a different kind of air from that which the muſcles had yielded; and from the little that I have done in this way, I cannot help thinking, that the experiments deſerve to be proſecuted farther.

From the *craſſamentum of blood*, with ſpirit of nitre, I got great plenty of air, part of which was fixed, but no part nitrous. At laſt the air was turbid; and, as is uſual in this caſe, a greater proportion of it was fixed air. Towards the laſt alſo, when the blood was completely diſſolved, the air was produced irregularly; for after an interval of about a quarter of a minute, there would be a ſudden guſh of about a quarter of an ounce-meaſure of air; but between theſe intervals the produce was equable.

Spirit of nitre put to the *ſerum of blood*, immediately turns it into a white coagulum. This yielded leſs air than moſt other ſubſtances, treated in the ſame manner. Part of it was fixed air, precipitating lime in lime-water, and the reſiduum was not nitrous, and extinguiſhed a candle without any particular appearance.

Milk was alſo immediately coagulated by a mixture of ſtrong ſpirit of nitre, and yielded air, one-third of which was fixed, precipitating lime in lime-water; and the remainder was ſo far nitrous, that two meaſures of common air, and one of this, occupied the ſpace of 2 $\frac{1}{4}$ meaſures.

From *cheeſe*, which was pretty old, I got air, a great part of which was fixed, and the remainder conſiderably nitrous.

Mutton-gravy, with ſtrong ſpirit of nitre, gave but little air, perhaps twenty times as much as its bulk. It was in part fixed, and the reſiduum not ſenſibly *nitrous*.

It has been ſeen, in a preceding ſection, that all *oily matters*, of a vegetable nature, yield nitrous air in very great plenty, and that the produce is exceedingly rapid; ſo that many precautions are neceſſary in conducting the experiments. On this account I began to uſe the ſame in my attempts to get air from *Hog's-lard*, but found them to be altogether unneceſſary: for this ſubſtance is but little affected by very ſtrong and hot ſpirit of nitre, on the ſurface of which it continues fluid, and yields but little air, perhaps four times its bulk. Part of this was fixed air, precipitating lime in lime-water, and the remainder was ſo far nitrous, that two meaſures of common air, and one of this, occupied the ſpace of leſs than two meaſures; that is, it was almoſt as ſtrongly nitrous as that which is produced from metals.

It is ſomething remarkable, that, of all animal ſubſtances on which I have made the experiment, that part which ſeems to be the moſt remote from a vegetable nature, and is peculiar to animals, ſhould approach the neareſt to the nature of a vegetable in the air which it yields when diſſolved in ſpirit of nitre. This is the *medullary ſubſtance of the brain.*

From part of the *brain of a ſheep*, diſſolved in ſtrong ſpirit of nitre, I got a quantity of air, about half of which was fixed air, precipitating lime in lime-water, and the remainder was ſo far nitrous, that two meaſures of common air, and one of this, occupied the ſpace of 2 $\frac{1}{4}$ meaſures. When it was completely diſſolved, and by a ſtrong heat, the air came over very turbid, and a candle burned in it with a lambent greeniſh flame.

I repeated the experiment with part of the ſame brain that was *boiled*, and with the ſame reſult; except that I did not continue the proceſs ſo long. The reſiduum of this air, when the fixed air was waſhed out of it, was ſo much nitrous, that two meaſures of common air, and one of this, occupied the ſpace of $2\frac{1}{5}$ meaſures. This I tried with the laſt of the three portions of air that I took. The firſt and ſecond were not ſo highly nitrous; and yet I am confident that all the three portions were wholly the produce of the ſolution, both the phial and the tube being filled with bubbles, in the form of froth, before I took any air at all.

After I had made theſe experiments, it occurred to me, that poſſibly, this difference in the produce of air from vegetable and animal ſubſtances might ariſe from ſome difference in the ſpirit of nitre. But though I found that, in conſequenc of the acid being more concentrated, or more diluted with water, a real difference was occaſioned in the air, ſtill very much depended upon the ſubſtances themſelves, as will appear from the following experiments.

A piece of *boiled mutton*, diſſolved in very ſtrong ſpirit of nitre, yielded air, which was partly fixed, with the reſiduum ſo far nitrous, that two meaſures of common air, and one of this, occupied the ſpace of $2\frac{1}{3}$ meaſures. Diſſolving a quantity of the ſame mutton, in the ſame ſpirit of nitre, diluted with an equal quantity of diſtilled water, I procured air, which was not half ſo much nitrous as that in the preceding experiment. With the ſame reſult I alſo made this experiment with the *white of an egg*, which gave air much leſs nitrous when diſſolved in a diluted ſpirit of nitre, than in the former caſe.

In order farther to ſatisfy myſelf, whether the reſult would not be the ſame with *vegetable* ſubſtances alſo, I took ſome pieces of very *dry old oak*, and diſſolved them in exceedingly weak ſpirit of nitre. I alſo cauſed the air, by means of heat, to be produced very rapidly; in which caſe the air is generally leſs nitrous, at leaſt toward the cloſe of an experiment, as the reader will have obſerved: but when the fixed air was waſhed out of it, the reſiduum was almoſt as ſtrongly nitrous as any air that is produced by the ſolution of metals.

SECTION IX.

Miſcellaneous Experiments relating to NITRE, *the* NITROUS ACID *and* NITROUS AIR.

I have more than once recommended the ſtudy of the *nitrous acid*, and of its various combinations, as promiſing a fund of valuable diſcoveries, looking far into the conſtitution of nature; and I flatter myſelf, that even my own experiments relating to this ſubject, recited in this volume, will be thought to have ſufficiently verified the obſervation. But I conſider this ample field of inquiry as barely opening, and that much more remains to be done; and conſidering how eaſily this rich mine has been worked hitherto, I think one may fairly preſume that it will ſtill abundantly reward the diligent labourer.

It has been the opinion of ſeveral eminent chymiſts, that there is but one *primitive acid*; that all the different acids with which we are acquainted, are only different modifications or combinations of it; and that the *nitrous acid*, in particular, differs from the reſt by a more intimate union of phlogiſton with it.

The celebrated Mr. Stahl obſerved, that by diſtilling iron in the marine acid, ſome nitrous acid was produced. Mr. Woulfe informs me, that he has done the ſame by a proceſs different from that of Stahl; and alſo that he has converted the acid of nitre into the marine acid, which I believe was never done before.

This I conſider as a capital diſcovery; and whenever this excellent chymiſt ſhall think proper to publiſh his proceſs, it will, I doubt not, be a great means of advancing the bounds of natural knowledge.

The relation that the nitrous acid bears to phlogiſton is to me, I acknowledge, a great myſtery. That this acid always contains phlogiſton is very evident; and yet, ſuch is its avidity, as we may ſay, for more, that it takes phlogiſton from moſt other ſubſtances. It is, I preſume, by means of this property, that many ſubſtances into which the nitrous acid enters, can burn without the aſſiſtance of common air; and I now ſuſpect, that it is by the ſame property that common air itſelf (which I think I have proved to conſiſt of nitrous acid and earth) is capable of ſuſtaining both flame and animal life.

I have ſufficient proof, however, that the nitrous acid, both when combined, as it generally is, with water, and likewiſe when exhibited in the form of vapour, or air, is ſo loaded with phlogiſton, as to be capable of phlogiſticating both the common air, and the nitrous air, which are expoſed to it. This I think to be a fact of a pretty extraordinary kind; at leaſt it appeared ſo much ſo to me, that I had expected the very contrary effect from the experiments that I made upon it; having imagined that, ſince the nitrous acid conſtitutes the pureſt of all the kinds of air, common air wanted nothing more than a greater proportion of this acid to become dephlogiſticated air; and thus I was in hopes of being maſter of a proceſs, by which I could not only reſtore vitiated air to its original purity, but even improve the purity of common air itſelf. And the attempt is of ſuch a nature, that I am far from thinking that any perſon needs to deſpair of ſucceeding in it, though the method which I took to accompliſh it did not anſwer, but had a contrary effect.

Among my random experiments to reſtore vitiated air, I have mentioned my having expoſed it to the fumes of ſmoking ſpirit of nitre, but without any effect; vol. I. p. 75. This was only ſpirit of nitre in the common heat of the atmoſphere, in a ſmall quantity, and for no long ſpace of time. I therefore conſidered the experiment as inadequate to the purpoſe of it; and when I had got my phials with ground-ſtopples, I introduced the tube of one of them, into which I had put ſome ſtrong ſpirit of nitre, under the edge of a ſmall jar, filled with air that had been injured by putrefaction about a year before; and making it boil, forced the hot fumes to riſe into, and mix with it, for a conſiderable time, till the acid ſeemed to be almoſt expelled from it; but I could not perceive that the air was ſenſibly altered by this operation. It was no more diminiſhed by nitrous air than it had been before.

It was on the very ſame day on which I made the preceding experiment, that it occurred to me that I had, at that very time, a very good opportunity of aſcertaining the effect of the fumes of ſpirit of nitre on common air, by means of a quantity of ſtrong ſmoking ſpirit of nitre, made at the Apothecary's-Hall, contained in a large phial, one-fourth part of which was full, and which had not been opened for half a year; ſo that all the incloſed air, which was three-fourths of the contents of the phial, had been expoſed to the vapour of the ſpirit of nitre all that time.

By loſing the ſpirit of nitre, I might have transferred this air into another veſſel, without any mixture of common air; but not chuſing to do that, I poured it into another phial, by which means I got a mixture, three-fourths of which was the air from the phial, and one-fourth atmoſpherical air, for which I had to make an allowance. Examining this mixture by the teſt of nitrous air, I found that two meaſures of it, and one of nitrous air, occupied the ſpace of 2 $\frac{1}{2}$ meaſures, and a candle would not burn in it; ſo that the ſpirit of nitre muſt have imparted phlogiſton to the air which had been expoſed to it, and in ſo great a degree, that it had become almoſt perfectly noxious; as may be eaſily concluded, by allowing for the mixture of common and wholeſome air with it in this experiment.

That *acid* fumes, as ſuch, have not this effect upon common air, I, at the ſame time, aſcertained, by making the ſame experiment on the air of the phial which had contained the ſtrongeſt ſpirit of ſalt; and, I believe, for a longer time. This, however, was in all reſpects as good as common air. This ſpirit of ſalt was procured from the Apothecary's-Hall, and ſmoked very much.

Afterwards, when I had contrived to fumigate different kinds of air, with the vapour of ſpirit of nitre, by a ſingular kind of proceſs, which will be mentioned below, I found that it had no effect whatever upon common air; for in this caſe, I believe, it contained very little phlogiſton: but the experiment was not tried to the greateſt advantage.

I was not much more diſappointed in my expectations from this experiment than I was in finding that air was injured by being expoſed to freſh-melted nitre. I had been led to make this experiment by obſerving that nitre, when it is fuſed by heat, yields air. Seeing this, I had the curioſity to try whether it would recover the air it had loſt by being expoſed to the common air, and at the ſame time to obſerve, what effect this expoſure would have upon the common air, in order to judge what it was that nitre, in thoſe circumſtances, took from the air. And I find that common air, expoſed to nitre in theſe circumſtances, is a little injured, but with ſuch circumſtances attending the *proof* of it, as I had never obſerved before, and which I cannot well account for. The facts were as follow.

I melted about an ounce of ſalt-petre in a crucible, till all the air ſeemed to be expelled from it, and immediately placed it under a receiver ſtanding in water, where it preſently became ſolid. The next morning I examined the air in which it had ſtood, and found it to be not ſo good as common air. It was diminiſhed about $\frac{1}{20}$ leſs than an equal quantity of common air, which I tried at the ſame time, and with the ſame nitrous air. I repeated the experiment ſeveral times, with the ſame reſult.

It was remarkable, however, that after theſe mixtures had remained a day and a night, they approached nearer to an equality. This alſo I obſerved more than once, and am much ſurprized at the fact. It ſhould ſeem that the air to which this nitre had been expoſed was not abſolutely ſo much injured as the firſt mixture of nitrous air with it would ſhow; but that its conſtitution was ſo much altered, that it required more time for the phlogiſton diſcharged from the nitrous air to act upon it.

Afterwards I melted ſome ſalt-petre in a glaſs-phial, and the veſſel being broken by the expanſion of the nitre in cooling, I expoſed the nitre to a quantity of air confined by water, ſo that the common air had acceſs to it on all ſides; whereas, in the former experiment, it had been contiguous to it at its ſurface only. After about a week I examined this air, and immediately after by the trial with nitrous air, found it to be conſiderably worſe than common air; two meaſures of it, and one of nitrous air, occupying the ſpace of two meaſures only: whereas the mixture of common air made at the ſame time, and with part of the ſame quantity of nitrous air, was diminiſhed as much as uſual. I did not carry this experiment any farther, as I did with another quantity of common air, which had likewiſe been expoſed about a week to melted ſalt-petre in the ſame circumſtances.

Two meaſures of this air, and one of nitrous, at firſt occupied the ſpace of little more than two meaſures; but it kept continually approaching to the degree of diminution of a mixture of common air made at the ſame time, till after four days the difference between them was very ſmall. Whether after a longer time theſe two mixtures would have been reduced to the very ſame dimenſions, I cannot tell. I made no more experiments of this kind, nor have I, in any other reſpect, purſued this ſingular kind of fact any farther.

In my former publications, I ſaid I had no doubt but that the nitrous acid might be exhibited in the form of air, and that experiments might be made upon it with a great

proſpect

proſpect of making conſiderable diſcoveries, provided that any fluid ſubſtance could be found capable of confining it. I have ſince made ſeveral attempts to diveſt this acid of the water with which it is generally combined; but though I have been favoured by ſome unexpected circumſtances, I have been far from ſucceeding to my wiſh.

That this acid is capable of exiſting in this dry form, I preſently ſatisfied myſelf by an attempt to expel air from it, by the ſame proceſs by which I had before expelled the marine acid air, from ſpirit of ſalt; viz. by heating the fluid in a phial, and receiving the air in quickſilver. For though the acid vapour very ſoon united with the quickſilver, yet the jar in which it was received being narrow, the ſaline cruſt, which was formed on the ſurface of the quickſilver, impeded the action of the acid upon it, till I had an opportunity of admitting water to the air that I had produced, and of ſatisfying myſelf, by its abſorption, of its being a real *acid air*, having an affinity with water, ſimilar to other acid airs.

In the firſt experiment that I made of this kind, the redneſs of the air did not appear immediately; but after ſome time, when it might be preſumed that the nitrous vapour had produced nitrous air, by a ſolution of the quickſilver; and the redneſs, I ſuppoſe to have been the effect of the mixture of this newly-generated nitrous air, with that portion of common air, which had been contained in the upper part of the phial, and which had been expelled by the acid vapour. I did not admit water to this air till after an hour; and even then it was ſenſibly diminiſhed; ſome of the acid air not having been ſeized by the quickſilver. The laſt time that I made this experiment, in which I produced about two ounce-meaſures of air, I admitted water to it as quickly as I could, and then one-third of the whole was imbibed by it.

In my account of the proceſs to procure dephlogiſticated air from calcined flint, and alſo from talck, I have obſerved that between the produce of the phlogiſticated and dephlogiſticated air, there is a conſiderable interval, in which nothing comes over but the pure *vapour of the acid*, which is inſtantly and wholly imbibed by water. This circumſtance gave me a fine and unexpected opportunity of making ſome experiments upon this vapour. For the orifice of the tube through which it was tranſmitted being plunged in water, and bending conſiderably upwards, I could eaſily put over it phials filled with any kind of air that could bear to be confined by water; and the end of the tube riſing a conſiderable way within the phial, the vapour muſt neceſſarily come into immediate contact with the air contained in it.

The firſt experiment that I made upon this vapour, in theſe circumſtances, was with *nitrous air*; and it appeared to have the ſame effect upon it that had been produced by liver of ſulphur, viz. diminiſhing it till it was no more capable of affecting common air; and the operation was exceedingly quick. Indeed the whole progreſs of this experiment is not a little remarkable. The moment that the phial of nitrous air was expoſed to this vapour, it became white, then tranſparent, then red; and, laſtly, tranſparent again. I took one quantity of this air, when the whiteneſs had juſt gone off; and found that it was but little different from pure nitrous air, diminiſhing common air almoſt as much. Taking another phial when it was quite red, one-third of the quantity had diſappeared, and its power of diminiſhing common air was about one-half of what it had been. I then let another phial remain expoſed to this vapour, till I perceived that the diminution would go no farther; when only $\frac{1}{20}$ of the original quantity remained, and this did not affect common air at all.

When this proceſs is quick, that is, when the nitrous vapour comes very faſt, the whiteneſs preceding the redneſs, on mixing the nitrous vapour with the nitrous air, can hardly be perceived, and the veſſel containing the air becomes exceedingly hot, as well as the tube through which it is tranſmitted. I obſerved that the veſſel containing nitrous air continued exceedingly red for about a minute, without any viſible change of dimenſions in the air; after which it was ſuddenly diminiſhed to about one-fourth of its original quantity, which reſembles the proceſs of the effervеſcence of iron-filings and brimſtone, deſcribed vol. I. p. 118.

I expoſed to this nitrous acid vapour, *common air*, *inflammable air*, and *fixed air*, and all of them for a conſiderable time, without making the leaſt ſenſible alteration in any of them. It is poſſible that a longer continuance of the proceſs might have affected them; but a great deal leſs time was abundantly ſufficient for this acid vapour to produce its utmoſt effect upon nitrous air. It ſhould ſeem, therefore, that though this acid vapour contained phlogiſton enough to phlogiſticate, preſently and completely, a quantity of nitrous air, it does not contain enough to phlogiſticate common air, at leaſt, that it requires either more time to effect this purpoſe, or a different mode of application.

As phlogiſton had produced no effect upon fixed air, except in one particular caſe, viz. from the effervecence of iron-filings and brimſtone, I did not abſolutely expect that it would have been affected in theſe circumſtances. Beſides, I only expoſed the fixed air to this vapour as it was expelled from the phial by the flame of a candle, when the vapour is not ſo copious as when it is expelled by a ſtrong ſand-heat, ſurrounding the whole phial placed in a crucible.

In the courſe of the experiments, I thought I ſaw reaſon to conclude that the nitrous acid air is naturally *colourleſs*, like the other acid airs. For I obſerved that, though the inſide of the phial, and alſo of the tube, was very red, during the tranſmiſſion of both the phlogiſticated and dephlogiſticated air, yet that in the intermediate ſtate, when the pure acid came over, all the inſide of the phial was tranſparent; or if there was any ſenſible colour, it was of a whitiſh caſt. At the ſame time it was obſervable, that this acid vapour, mixing with any other kind of air, produced a red colour.

As there was this redneſs in inflammable air, and other kinds of air, for ſome time after this vapour was admitted to them, and they afterwards became tranſparent, I expected that ſome alteration would have been made in them; but I was diſappointed.

I would here obſerve, that the young operator ought to be very cautious in conducting this proceſs, and eſpecially to take care that the tube through which this acid vapour is tranſmitted be ſufficiently wide; by which I mean that the hollow part of it ſhould be about one-tenth, or one-twelfth of an inch in diameter. When, at one time, I was ſo incautious as to make uſe of a tube much ſmaller than this, almoſt capillary, ſome particles of the flint, as I ſuppoſe, got into it, and ſtopped it up. However, there was a violent exploſion of the phial, and of all its contents, by which I was expoſed to ſome danger: but providentially, at this time, as upon many other occaſions, I eſcaped without any hurt. But, in ſuch a kind of buſineſs as this, nothing can be expected to be done without ſuch riſques.

In my former publication, p. 126, I have obſerved that I got little or no air by diſſolving *lead* in ſpirit of nitre. I have ſince, however, made another attempt of this kind, and with a little better ſucceſs. I poured ſmoking ſpirit of nitre into a phial with a groundſtopple and tube, fig. *c*, containing 1 ½ ounce-meaſures, filled with ſmall leaden ſhot, ſo as to leave no common air at all, either in the phial or in the tube; and I placed it ſo as to receive the air that might come from it in water.

After waiting an hour, in which little or no air was produced, I applied the flame of a candle, though not very near to it, and in theſe circumſtances I got about an ounce-meaſure of air: but upon ſome water ruſhing into the phial, while the candle was withdrawn, air was produced very plentifully. I collected, in all, about a quarter of a pint, and might probably have got much more; but that the ſalt formed by the ſolution of the lead had ſo nearly cloſed up the tube, that I thought proper to diſcontinue the proceſs. The air, both of the firſt and of the laſt produce, was of the ſame quality, and ſo far nitrous, that two meaſures of common air, and one of this, occupied the ſpace of two meaſures only; excepting that the very firſt and very laſt produce, mixed with common air, took up a little more room than that which I got in the middle of the proceſs. When the air was produced very faſt, it was exceedingly turbid, as if it had been filled with a white powder.

In my former publication, the reader will have ſeen the reſult of ſeveral proceſſes in which nitrous air was phlogiſticated with iron and liver of ſulphur, in conſequence of which a candle would burn in it, either naturally, as in common air, or with a beautifully enlarged flame. As this air, in ſome reſpects, reſembles common air, though it be noxious, it occurred to me, that it might be poſſible, by means of ſome ingredients, to make it in all reſpects common air; and with this, and other views, I, at different times, filled ſeveral phials with nitrous air, putting to it iron or liver of ſulphur to phlogiſticate it, and alſo pieces of chalk, or a mixture of fixed air, in order to ſupply it with that ingredient, which it is well known the atmoſphere contains; and in other reſpects alſo I varied theſe preparations, in order to have the greater chance of ſucceeding with reſpect to the object of the experiment. Theſe projects, the reader will eaſily imagine, were antecedent to my diſcovery of the real conſtitution of the atmoſphere, as explained in a preceding ſection. However, as the proceſſes took up a good deal of time, and ſome perſons, intent upon theſe purſuits, may wiſh to know what was the reſult of them, unſucceſsful as they were with reſpect to my main object, I ſhall here recite the particulars of the principal of them, in the order in which they were made.

The reader may alſo have obſerved, that, in one particular caſe, mentioned vol I. p. 220, a quantity of nitrous air, which had been expoſed two months to ſome iron nails in quickſilver, was diminiſhed by a mixture of freſh nitrous air. This I find in my regiſter, written at the time of obſervation, and therefore can hardly doubt but that I muſt have obſerved that appearance, which is an indication of a conſiderable degree of purity in the air, and of its fitneſs for reſpiration. But as in none of the following experiments I could get the ſame appearance, I ſuſpect that I muſt, ſome way or other, have deceived myſelf on the former occaſion. It muſt be obſerved, however, that it by no means follows, that becauſe we cannot, in a courſe of experiments, produce the ſame appearance in what we *imagine* to be the ſame circumſtances, that we were therefore deceived with reſpect to the appearance itſelf; becauſe nothing is more common than for perſons to be deceived with reſpect to what they *imagine* to be the ſame circumſtances in an experiment.

June 4, 1774. Two quantities of nitrous air, which had ſtood above four months in contact with iron in water, juſt extinguiſhed a candle.

July 25. A candle burned with an enlarged flame; but not more than double, in nitrous air, which had been in contact with iron in quickſilver, about ſix months. The appearance was the ſame when the candle was dipped into it, both before it was once paſſed through the water, and afterwards. Water being admitted to the remainder of this air, it began to be abſorbed as uſual.

March 2, 1775. Nitrous air, which had been confined above a year in contact with iron, ſtanding in water, was, in all reſpects, like phlogiſticated common air: it neither diminiſhed common air, nor was diminiſhed by nitrous air, and extinguiſhed a candle. It had alſo the faint ſmell of phlogiſticated air. The more ruſty iron is, the faſter it diminiſhes nitrous air, which looks as if it took phlogiſton from the nitrous air, rather than communicated any to it.

March 4. In about one day, and without any heat, about one-third of a given quantity of nitrous air was imbibed by liver of ſulphur. In the remainder a candle burned with an enlarged flame; but it was not at all diminiſhed by freſh nitrous air.

March 6. Nitrous air, expoſed to liver of ſulphur and chalk, exhibited the ſame phenomena as if no chalk had been put to it: it admitted a candle to burn in it with an enlarged flame, was not diminiſhed by nitrous air, and extinguiſhed a candle, after a very little agitation in water.

March 10. A quantity of one-half nitrous, and one-half fixed air, which had been in contact with iron, was reduced one-third in its dimenſions, and the remainder admitted a candle to burn in it with an enlarged flame, but was not diminiſhed by nitrous air.

May 7. I examined ſeveral quantities of nitrous air, and mixtures of nitrous and fixed air, which had ſtood expoſed in quickſilver, to iron, or to iron that had ruſted in nitrous air, about two months. None of them were diminiſhed by nitrous air, or diminiſhed common air. In general they extinguiſhed a candle;

dle; but, in one of them, a candle burned naturally when the fixed air had been waſhed out of it in water. One quantity of nitrous air, which had been expoſed to iron that had ruſted in nitrous air, was diminiſhed about one-tenth, but was very little changed; for it diminiſhed common air almoſt as much as freſh-made nitrous air. Another quantity of nitrous air, which had been expoſed to iron-nails, diminiſhed common air rather leſs than the preceding.

SECTION X.

Some Obſervations on COMMON AIR.

It is generally ſuppoſed, and perhaps with reaſon, that the uſe of ſome metals is much ſafer than others, on account of ſome *effluvia* iſſuing from them. Copper and lead, for inſtance, are thought to have ſome noxious quality of this kind; whereas iron is thought to be perfectly harmleſs, in every ſhape, as Dr. Franklin humorouſly obſerves, excepting in that of a weapon. My experiments on the diminution of air, by paint, conſiſting of white lead and oil, by which it is rendered perfectly noxious, have been interpreted ſo as to favour that opinion; and, in my account of thoſe experiments, I had myſelf aſcribed the effect to the phlogiſton remaining in the white lead.

I had then, however, obſerved, that air is as much diminiſhed, and conſequently is rendered as completely noxious, by the calcination of *tin*, as by that of lead; and tin, I believe, does not lie under any peculiar odium.

Indeed, if my hypotheſis be well founded (and ſo many facts have occurred to my obſervation in ſupport of it, that I do not ſee how it can be called in queſtion) viz. that it is phlogiſton that diminiſhes air, and renders it noxious; it muſt be a matter of indifference from which of the metals it is diſcharged, and the calcination of any of them muſt render the air in which the calcination is made equally noxious. Other facts have occurred in the courſe of my late obſervations that farther confirm this opinion.

When I was making the experiments on the extraction of inflammable air from iron, I found, that if the quantity of air was conſiderable, the throwing of the focus of the burning lens upon iron-filings incloſed in it, had no other effect than to diminiſh the air, and make it noxious; which it did, to as great a degree as the calcination of lead or tin had done: for after this it made no efferveſcence, and was no farther diminiſhed, by nitrous air. After this, I make no doubt, but that if the proceſs had been continued a ſufficient time, there would have been an increaſe of the quantity of air, by the production of inflammable air; but the firſt effect of the diſcharge of phlogiſton from the iron was the phlogiſticating and diminiſhing of the common air.

I even found that air would be injured by having iron confined in it for a conſiderable time. To try this, I filled a phial, containing common air, full of nails, the 18th of December, 1773, and left it inverted in a veſſel of water till the 2d of March, 1775, when I found that it was diminiſhed one-fifth of its bulk, and was not in the leaſt diminiſhed by a a mixture of nitrous air; ſo that it muſt have been perfectly noxious.

I have alſo ſeen reaſon to think, that my former opinion concerning the cauſe of the diminution of air by paint, viz. that it was effected by the diſcharge of phlogiſton from the *white lead*, was not well founded. For in making my experiments with *red lead*, having found before, that a compoſition of which turpentine made a part, had diminiſhed common air (vol. I. p. 179) and therefore, ſuſpecting that poſſibly it might be the oil of turpentine, and not the white lead, that contributed moſt to the diminution of the air, I got a ſmall quantity of *paint made with red lead*, mixed in the uſual manner; and found, that when I had daubed pieces of paper with this paint, and covered them with a jar ſtanding in water, the air was diminiſhed juſt as before; ſo that it was probably the phlogiſtic effluvia of the *oils*, and not of the *lead*, that produced this effect.

In my former publication, I obſerved that air, which had been expoſed to the effluvia of the common *red cement*, was injured by it. For this purpoſe, I had covered all the inſide of a phial with it, and placed it, inverted, in a veſſel of water. Since that time I have repeated, or rather continued the experiment, letting the ſame veſſel ſtand about nine months in that ſituation; when, upon examining it, I found it was diminiſhed one-fifth of its bulk, and that it was not at all affected by nitrous air.

Having had ſo much to do with *red lead*, in the courſe of my late experiments, I had the curioſity to try what would be the effect of that proceſs by which the *grey calx of lead* is converted into red lead. The ſimple calcination of lead, I knew to be the diſcharge of phlogiſton, by which the air contiguous to it is diminiſhed; and, as far as I have had an opportunity of obſerving, the continuance of the ſame proceſs, by which the grey calx is converted into red lead, is of the ſame nature, viz. a farther diſcharge of phlogiſton, and conſequently a diminution of the air. For I threw the focus of a burning lens upon a ſmall quantity of the grey calx of lead, placed under a receiver ſtanding in water; and though this operation did not make it abſolutely red lead, it gave it a reddiſh tinge; and I found, by the teſt of nitrous air, that the air in which the experiment was performed was conſiderably injured, being not nearly ſo much affected by nitrous air as common air is.

As common air contains a conſiderable proportion of fixed air, I was deſirous, from the time that I began my experiments on air, to extract the fixed air from a given quantity of atmoſpherical air, to ſee what it would be when deprived of that ingredient. I had found, indeed, that fixed air is precipitated from common air in phlogiſtic proceſſes, and eſpecially by the electric ſpark; but this is a complex proceſs; for at the ſame time that fixed air is diſcharged, phlogiſton is received by the air. But nothing that I have yet thought of has ſucceeded ſimply to extract the fixed air.

As quicklime has a great affinity with fixed air, I thought it poſſible that confining a large quantity of quicklime in a ſmall quantity of air might, in time, produce ſome effect of this kind; but the experiment did not anſwer, though I crowded a great number of ſmall pieces of the beſt quicklime that I could procure, into a phial, which I left inverted in a baſon of quickſilver a whole week. It did not appear that the incloſed air was at all ſenſibly affected. Had it been ſo in the ſmalleſt degree, I ſhould have repeated the proceſs, and have allowed it more time.

Having diſcovered that vegetation reſtores, to a conſiderable degree of purity, air that had been injured by reſpiration or putrefaction; and alſo that agitation in water produces the ſame effect, I conjectured that the phlogiſtic matter, abſorbed by the water, might be imbibed by plants, as well as form other combinations with ſubſtances under the water. A curious fact, which has ſince been communicated to me, very much favours this ſuppoſition.

Mr. Garrick was ſo obliging as to give me the firſt intimation of it, and Mr. Walker, the ingenious author of a late Engliſh Dictionary, from whom he received the account, was pleaſed to take ſome pains in making farther inquiries into it for my uſe. He informed me that Mr. Bremner, who keeps a muſic-ſhop oppoſite to Somerſet-houſe, was at Harwich, waiting for the packet; and obſerved that a reſervoir at the principal inn was very foul on the ſides. This made him aſk the inn-keeper why he did not clean it out; who immediately anſwered, that he had done ſo once, but would not any more; for that after cleanſing the reſervoir, the water which was caught in it grew fetid, and unfit for uſe; and that it did not recover its ſweetneſs till the ſides and bottom of the reſervoir grew very foul again. Mr. Walker queſtioned Mr. Bremner, whether there were any vegetables growing at the ſides and bottom of it; but of this he could not be poſitive. However, as he ſaid it was covered with a *green ſubſtance*, which is known to be vegetable matter (and indeed nothing elſe could well adhere to the *ſides*, as well as to the bottom of the reſervoir) I think it will be deemed probable, that it was this vegetating matter that preſerved the water ſweet, imbibing the phlogiſtic matter that was diſcharged in its tendency to putrefaction.

I ſhall be happy, if the mention of this fact ſhould excite an attention to things of this nature. Trifling as they ſeem to be, they have, in a philoſophical view, the greateſt dignity and importance; ſerving to explain ſome of the moſt ſtriking phenomena in nature, reſpecting the general plan and conſtitution of the ſyſtem, and the relation that one part of it bears to another.

SECTION XI.

Of the Fluor Acid Air.

The philoſophical part of the world have, of late, been highly gratified by the diſcovery of what was imagined to be *a new mineral acid*, contained in a ſubſtance which the chymiſts diſtinguiſh by the name of *fluor*; but many of my readers will underſtand me better, when I inform them, that it is of that ſpecies of ſubſtance, which, with us, is called *the Derbyſhire ſpar*; and of which, at preſent, vaſes and other ornaments for chimnies, are uſually made. The acid is expelled from this ſubſtance by oil of vitriol, and has peculiar properties, as remarkable as any of the other three mineral acids which we were acquainted with before.

This curious diſcovery was made by Mr. Scheele, a Swede; from which circumſtance the acid is often diſtinguiſhed by the name of the *Swediſh acid.* His method of operating upon this ſubſtance, and likewiſe that of all who have ſucceeded him in the inquiry, was to diſtil it in glaſs-veſſels, as in the proceſs of making ſpirit of nitre from ſalt-petre; and the moſt remarkable facts that have been obſerved concerning it are, that the veſſels in which the diſtillation is made are apt to be corroded; ſo that holes will be made quite through them; and that when there is water in the recipient, the ſurface of it will be covered with a cruſt, of a friable ſtony matter.

This cruſt, which I ſhall diſtinguiſh by the name of *the fluor cruſt*, Mr. Scheele ſuppoſed to be *quartz*; and therefore concluded that this acid and water were the conſtituent parts of that foſſil. On the other hand, Mr. Boulanger, who has taken a great deal of pains with this ſubject, is of opinion that this new acid is only the *acid of ſalt*, combined with an earthy ſubſtance. For this opinion he advances various reaſons; but does not pretend to be able to produce any deciſive proof. The reſult of my own experiments, I think, clearly prove, that the fluor acid is the *acid of vitriol*, charged with ſo much phlogiſton as is neceſſary to its taking the form of air, and alſo with much of the earthy matter of the ſpar.

As ſoon as I had exhibited one of the acids in the form of *air*, I had no doubt but that all the acids might be exhibited in the ſame manner, and this among the reſt; but I imagined that I ſhould find great difficulty in procuring the foſſil that contains it; ſuppoſing that it had only been found in Sweden; and I ſhould probably have continued in this incapacity for making the following experiments, had I not been relieved by Mr. Woulfe, who, upon my inquiry concerning it, not only explained to me what the ſubſtance was, but immediately furniſhed me with a quantity of ſeveral kinds of it, ſufficient for my purpoſe. That with which my firſt experiments were made, was that which he called *the white phoſphoric ſpar, from Saxony*; but afterwards I made uſe of the Derbyſhire ſpar; and the pieces that I had by me were partly white, or yellowiſh, and partly purple.

All my advantage in the inveſtigation of this ſubject, has ariſen from my peculiar manner of conducting the experiments. For, by exhibiting the acid in the form of air, free from all moiſture, I had an opportunity of examining its nature and affinities with the greateſt eaſe and certainty. In this manner alſo, this ſpecies of air exhibits a variety of ſtriking phenomena, which cannot be produced in any other manner of operating upon it.

When I began theſe experiments, I followed the directions given by thoſe who had gone before me in the inveſtigation of this ſubject, and who had procured the acid in the common method of diſtillation, pounding the fluor (which I afterwards found not to be neceſſary) and pouring oil of vitriol upon it. This I did in a phial, to which was fitted a ground-ſtopple and tube, and immediately found, that, at firſt, without any heat, and afterwards with a very ſmall degree of it, air was produced in great plenty, perfectly tranſparent, and confined by quickſilver, like the other acid airs. The vapour, as it iſſued out of the tube into the open air, formed a permanent white cloud; no doubt, by attaching to itſelf the water that floated in the atmoſphere, and the ſmell of it was extremely pungent.

I had no ſooner produced this new kind of air, but I was eager to ſee the effect it would have on *water*, and to produce the ſtony cruſt formed by their union, as deſcribed by Mr. Scheele; and I was not diſappointed in my expectations. The moment the water came into contact with this air, the ſurface of it became white and opake, by a *ſtony film*, which, forming a ſeparation between the air above, and the water below it, conſiderably retarded the aſcent of the water, till the air, inſinuating itſelf through the pores and cracks of this cruſt, the water neceſſarily roſe as the air diminiſhed, and breaking the cruſt, preſented a new ſurface of water, which, like the former, was inſtantly covered with a freſh cruſt. Thus was one ſtony incruſtation formed after another, till every particle of the air was united to the water, and the different films being collected, and dried, formed a white powdery ſubſtance, generally a little acid to the taſte; but when waſhed in much pure water, was perfectly inſipid.

Few philoſophical experiments exhibit a more pleaſing appearance than this, which can only be made, by firſt producing the air confined by quickſilver, and then admitting a large body of water to it. Moſt perſons to whom I have ſhewn the experiment have been exceedingly ſtruck with it. It is exhibited to the moſt advantage, when the veſſel that contains the air is pretty wide, by which I mean about an inch in diameter. In this caſe the cruſt will often crack in the middle, and a ſmall jet of water ruſhing through the fiſſure, will, to appearance, be inſtantly converted into this ſtony ſubſtance, and look like a puff of white powder, riſing ſometimes an inch or two up into the air. Alſo the *cryſtallizations*, formed on the ſides of the veſſel, as the water riſes in it, make a very beautiful appearance.

The union of this acid air and water may alſo be exhibited in another manner, which, to ſome perſons, makes a ſtill more ſtriking experiment; viz. by admitting the air, as faſt as it is generated, to a large body of water reſting on quickſilver, inſtead of introducing the water to the air previouſly formed.

For this purpoſe, I uſually put two or three ounce-meaſures of water into a tall cylindrical jar, about an inch in diameter (ſuch as thoſe which I generally uſe as recipients of thoſe kinds of air that muſt be confined by quickſilver) and filling the remainder of the veſſel with quickſilver, I place it inverted in a baſon containing a quantity of the ſame fluid; ſo that the water, immediately riſing to the top, occupies the upper part of the veſſel, while the quickſilver occupies the lower part. I then introduce under it the end of the tube proceeding from the phial, which contains the materials for generating this air. It is, then, very pleaſing to obſerve, that the moment any bubble of air, after paſſing through the quickſilver, reaches the water, it is inſtantly, as it were, converted into a ſtone; but continuing hollow for a ſhort ſpace of time, generally riſes to the top of the water, in the form of a bubble, or a thin white film. If the ſucceſſion of bubbles be rapid, and they riſe freely to the top of the veſſel, through a large body of clear water (which, however, is not always the caſe, as they will ſometimes adhere to the upper ſurface of the quickſilver) I have met with few perſons who are ſoon weary of looking at it; and ſome could ſit by it almoſt a whole hour, and be agreeably amuſed all the time.

Every bubble of air, coming into contact with the water on every ſide at once, is like a bladder, being hollow within; but this ſlight cruſt ſoon burſting, the ſides collapſe, and it riſes to the top of the veſſel, in the form of a piece of thin white gauze; but the water ſoon penetrating every part of it, the whole maſs of theſe films becomes in a little time like a jelly, which continually thickens by the acceſſion of more films, till at length the whole body of water ſeems to become ſolid; ſo that, being fully ſaturated, eſpecially at the lower part, the air, finding no more moiſture within its reach, will fill all the lower part of the veſſel, expelling the quickſilver, while the water, in the form of a ſtiff jelly, occupies all the upper part of the veſſel.

As, for the purpoſes to be mentioned hereafter, I have repeated this experiment a great number of times, I have had an opportunity of obſerving a very great variety in the appearances which it exhibits. One is peculiarly pleaſing, but not very common. A large bubble of air will ſometimes adhere, by its lower part, to the ſurface of the quickſilver; and another bubble, riſing in the ſame place, before the lower part of the former has been cloſed, puſhes out the upper part of it, and advancing farther into the water, extends the bubble in length: another follows, and does the ſame, till at length a *tube* is formed (the ſides alſo growing thicker continually) extending from the quickſilver, to the top of the water. I have ſeen of them four inches in length; and others being formed cloſe to them, the whole veſſel has been almoſt filled with theſe tubes, adhering to one another, of different lengths, and not much unlike the appearance of the pipes that are placed in the front of an organ.

In leſs than an hour, I have frequently converted two or three ounce-meaſures of water into this ſolid maſs. When this is taken out of the veſſel, and preſſed, it will be found to contain a great deal of an acid liquor; the water impregnated with the acid having been intangled in the interſtices of the jelly, out of the reach of the air: and if this liquor be uſed in another proceſs, inſtead of pure water, more of it will ſeem to become ſolid, and the acid liquor will be concentrated every time.

By the repetition of this proceſs, an acid liquor may be procured of a very conſiderable degree of ſtrength. There ſeems, however, to be a limit to its ſtrength; for the acid is exceedingly volatile, as is evident from its extremely pungent ſmell; ſo that I have thought that I gained nothing by repeating the proceſs more than eight or ten times; becauſe it was impoſſible to transfer the water from one veſſel to another, but more acid would be loſt by evaporation, than would be acquired by another impregnation with the acid air.

Theſe appearances I explain, by ſuppoſing that the vitriolic acid, in uniting with the ſpar, is in part volatilized, by means of ſome phlogiſton contained in it, ſo as to form a vitriolic acid air; and that there is alſo combined with this air, a portion of the ſolid earthy part of the ſpar, which continues in a ſtate of ſolution, till, coming into contact with the water, the fluid unites with the acid, and the earth is precipitated. The reaſons on which I found this opinion will appear in due time; but to make my reader follow me, ſtep by ſtep, in my analytical progreſs, I muſt firſt acquaint him with the obſervations I made upon this acid air, in its compound ſtate, before the ſtony matter was ſeparated from it. For this union makes it quite another thing, giving it peculiar properties, which are not to be diſcovered in the pure acid air, diveſted of that ſtony matter; and therefore, though it be a compound, and may be analyzed into its conſtituent parts, it is ſufficiently intitled to a peculiar appellation, viz. that of *the Fluor acid air*.

Before I proceed to relate any of the experiments which I made with this acid air, I ſhall give a few directions and precautions, which may be uſeful to perſons on their firſt entrance upon this courſe.

1. The tube through which this acid vapour is conveyed ſhould not be very narrow, becauſe it is apt to be furred up, eſpecially when any phial, containing materials for the production of this air, has been uſed ſome time, and with a good deal of heat; owing, I ſuppoſe, to the hot air retaining in ſolution more of the ſtony matter than it can do when it is cold, and therefore depoſiting it as it is conveyed through the tube.

2. I began theſe experiments with phials which had ground-ſtopples and tubes, but ſoon found that it was too expenſive a mode of experimenting with this kind of air; for they were preſently corroded and ſpoiled. Afterwards,

terwards, therefore, I uſed only common phials, but the thickeſt that I could meet with; and ſtill ſeldom found that they would bear the experiment above an hour. Very frequently, the thickeſt phials that I could get would be worn quite through in a quarter of an hour, when the heat was conſiderable, and the production of the air rapid. This power of diſſolving glaſs is a very remarkable property of this air; but it ſeems to poſſeſs it only when it is hot, at leaſt in any conſiderable degree.

3. When I wiſhed to produce this air pretty faſt, I found it moſt convenient to pound the ſpar, and pour the oil of vitriol upon it, filling one-fourth of the phial with the ſpar, and leaving one-fourth of it for a ſpace in which the bubbles might expand themſelves, and break, ſo as not to carry any of the liquor into the tube. I now proceed to the particular experiments.

Dipping a lighted candle into a veſſel filled with the fluor acid air, it was extinguiſhed without any particular colour of the flame, which is obſervable in the marine acid air,

The mixture of any other of the acid airs, with *alkaline air*, makes ſo beautiful an experiment, that it was naturally one of the firſt experiments I thought of making with this new acid air. Accordingly, I got the appearance that I had expected; a white cloud being formed by the union of theſe two kinds of air. But the alkaline air did not mix ſo readily with this as with the other kinds of acid air; and which ſurprized me much at the time, the ſalt formed by the union of theſe two kinds of air was not ſoluble, either in water or ſpirit of wine. But, in fact, the proper *ſalt* formed by the union of theſe kinds of air was, no doubt, diſſolved in the water; that which remained undiſſolved being, as I conjecture, the *ſtony ſubſtance* only which had been held in ſolution in the acid air. This ſtony ſubſtance, being mixed with the acid air, is alſo probably the reaſon why the alkaline air does not mix ſo readily with it as with the other kinds of acid air; ſome time being requiſite to diſengage it from this ſtony ſubſtance, in order to its uniting with the alkaline air.

Nitrous air, mixed with this acid air, had no ſenſible effect upon it. Water abſorbed the acid air, and left the nitrous air poſſeſſed of its peculiar properties.

Having aſcertained the effect of water upon this acid air, I proceeded to try other *fluid ſubſtances.*

Spirit of wine imbibed this air as readily as water, but continued as limpid as ever; and when ſaturated with it, ſeemed to be no leſs inflammable than before.

Oil of turpentine, did not imbibe any of this air.

Vitriolic ether imbibed about twenty times its own bulk of it; but was not ſenſibly changed by the impregnation. The caſe was the ſame with *nitrous ether*. But the firſt time that I made the experiment with nitrous ether, I imagine a little water was mixed with it (as much as thoſe ſubſtances are capable of being mixed) for it coagulated as water had done, remaining in the middle of the tube, the acid air being both above and below it. This maſs of coagulated matter; which in colour and conſiſtence reſembled a brown jelly, being taken out of the veſſel, did not take fire at the approach of a candle; but when it had been expoſed to the air about half a minute, it grew hot, threw out a groſs ſmoke, and was preſently all evaporated. Part, however, of the ſame maſs, which had been dipped in water, did not grow hot, or evaporate, in the open air; and when expoſed to the fire, it burned to a white powdery ſubſtance. I imagine this effect to have been owing to a mixture,

ture of water; becauſe, with pure nitrous ether, I could not get another appearance of the kind.

Of *ſolid ſubſtances*, I found that this air had no effect upon *brimſtone*, *common ſalt*, *ſal ammoniac*, *iron*, *liver of ſulphur*, or *gum-lac*.

Charcoal abſorbed the whole of a quantity of this air, and contracted from it a ſtrong pungent ſmell. The *ruſt of iron* alſo abſorbed it in like manner.

Alum abſorbed this air pretty faſt, the ſurface of it being rendered white and opake. When it was taken out of the air, it looked moiſt, and was incapable of the operation of roaſting, like that which had been expoſed to alkaline air. This air having, no doubt, like the other, ſeized upon the water which enters into the compoſition of alum.

Quicklime and *chalk*, both abſorbed a little of this acid air; but the reſult was, in no reſpect, remarkable. The latter had been diſſolved by it, and had produced a quantity of fixed air, precipitating lime in lime-water.

In order to judge whether there was any foundation for the opinion of Mr. Boulanger, of this acid being the ſame with the *marine*, I put to it a piece of ſalt-petre, which I have obſerved to be readily diſſolved in the marine acid air; and I muſt own that appearances ſo much favoured his opinion, that I was at that time very much inclined to adopt it.

When the ſalt-petre had been for ſome time ſurrounded with this air, the air began to be diminiſhed, and the inſide of the veſſel was filled with red fumes, which continued about a week, the quickſilver riſing all the time, till only one-tenth of the air remained, and the inſide of the veſſel was covered with a whitiſh, probably a ſaline ſubſtance, produced by the ſolution of mercury. After this, the air becoming tranſparent, I examined it, and found it neither to affect common air, nor to be affected by nitrous air, and to extinguiſh a candle. Alſo, about one-fourth of it was readily abſorbed by water, and made lime-water turbid; ſo that, contrary to my expectation, a great part of the air muſt have been fixed air, and not nitrous. This experiment I did not repeat; but it ſeems to exhibit a fact deſerving particular attention, in the inveſtigation of the nature of fixed air.

I thought it might poſſibly contribute to decide the queſtion concerning the identity of this acid and the marine, if I put a quantity of the *fluor cruſt* to marine acid air; thinking that they might form an union, and conſtitute this fluor acid air: and, indeed, ſomething ſimilar to it was by this means produced; ſo that another cruſt was formed upon the admiſſion of water to it; but, in other reſpects, ſeveral circumſtances, which I cannot explain, attended the experiments. They were as follows.

To about two ounce-meaſures of marine acid air I put about a quarter of a grain of the fluor cruſt, and in about three days it had abſorbed about half an ounce meaſure of the air. Water being then admitted to it, left a quarter of an ounce-meaſure of air unabſorbed.

Afterwards I conveyed marine acid air to a pretty large quantity of the fluor cruſt, confined by quickſilver; and, as the air was imbibed, I continued to throw up more, till, after three or four days, that ſubſtance ſeemed to be fully ſaturated with the air. Then admitting water to it, it was abſorbed exactly like the fluor acid air: but I could not, at that time, very well diſtinguiſh the cruſt on the top of it, on account of the jar being almoſt filled with the cruſt, and part of it floating on the top of the water. About three-fourths of this air was abſorbed by the water; but what I thought very remarkable, air kept iſſuing from this fluor-cruſt, in large bubbles, till the quantity of air was doubled, and the jar was half-filled with it. This air neither affected common air, nor was affected by nitrous air, and it extinguiſhed a candle.

I repeated the experiment, with this only difference, that I admitted water to the air as ſoon as the fluor-cruſt ſeemed to be ſaturated; when the experiment being made in a wide jar, the cruſt on the ſurface of the water was as viſible as in the experiment with the fluor acid air itſelf. At this time, however, there was no generation of air from the ſaturated cruſt, as before, but a conſiderable quantity of air, unabſorbed by water, though I took care that the marine acid air was as pure as I could procure it.

Having a quantity of the fluor-cruſt ſaturated with marine acid air, I had the curioſity to pour ſome oil of vitriol upon it, in order to try whether the produce would be pure marine acid air, or a mixture of the two; and the latter ſeemed to be the caſe, though I think the marine acid prevailed in the mixture.

In this proceſs air was produced in great plenty, and the bubbles burſt in the receiver with a white cloud; but when water was admitted to it, it was abſorbed without any cruſt being formed upon its ſurface. In twenty-four hours a piece of ſalt-petre turned yellow in this air, and abſorbed about half an ounce-meaſure of it. What remained unabſorbed by water, was exceedingly ſtrong nitrous air, the ſpirit of nitre having been ſet looſe from the ſalt-petre by the marine acid air, and having diſſolved the quickſilver.

A piece of *borax*, in about a fortnight, abſorbed about two ounce-meaſures of this air, without leaving any reſiduum not abſorbed by water. The ſurface of the borax was become ſoft; but by waſhing it in water, the ſoft part was eaſily ſeparated from the reſt.

At the very beginning of my inveſtigation of this ſubject, I had a ſuſpicion that this new acid air might poſſibly be the *vitriolic acid air*, loaded with the ſparry cruſt; but not ſucceeding in the experiments which I thought muſt have decided the queſtion, I quitted that hypotheſis for ſome time. The experiments were theſe.

I threw the focus of a burning lens upon ſome pieces of the ſpar in vitriolic acid air, confined by quickſilver; thinking, that when it was hot, it might diſſolve ſome part of it, and thereby become the ſame thing with the fluor acid air. But though I continued this operation till the ſpar ſmoked, and filled the veſſel with a white fume, there was neither any addition made to the quantity of air, nor any change produced in the quality of it. When water was admited to it, no cruſt, as I had expected, was formed on the ſurface of it.

In order to try whether the fluor cruſt was the ſame thing with the ſpar, from which it had been produced, I got a quantity of it, and treated it in the ſame manner as I had treated the ſpar, pouring oil of vitriol upon it, and endeavouring to expel air from it. I preſently found, indeed, that it yielded great plenty of air; but not finding it to be the thing I was then in queſt of, viz. an acid air, by means of which a cruſt would be formed on the ſurface of the water admitted to it, I neglected to give ſufficient attention to it, or I might have been led to ſuſpect that this fluor-cruſt, like the fluor itſelf, contains ſo much phlogiſton, as, by incorporating with the oil of vitriol, to enable it to aſſume the form of air, and become the vitriolic acid air; though the earthy matter, not incorporating with it, it could not become the fluor acid air.

Water admitted to this acid air, procured from the fluor-cruſt by oil of vitriol, abſorbed it all, but without having any cruſt upon its ſurface.

Alkaline air united with the whole of this acid air, forming with it a white ſaline ſubſtance; and part of the inſide of the tube in which the mixture was made, was tinged with a deep yellow, or orange-colour, which diſappeared after a few hours expoſure to the open air. This I have obſerved to be the caſe with the vitriolic acid air.

This air did not at all affect *ſalt-petre* or *borax*.

Had I, however, proſecuted theſe experiments farther, and have found, as, I doubt not, I ſhould have done, that this acid air, procured by oil of vitriol from the fluor-cruſt, was genuine vitriolic acid air, it would have proved no more than that this fluor-cruſt contains phlogiſton, and in ſuch a ſtate as to combine with oil of vitriol by heat, and enable it to take the form of air. They would not have proved, that the air procured from the fluor itſelf was of that nature: for it might have been ſaid, that the peculiar acid of the fluor had been expelled before.

To make the *experimentum crucis* in this caſe, I ſaturated a quantity of water with the fluor acid air, preſſing out the ſtony matter with which it was filled at each proceſs, and impregnating it over and over again. When it appeared to be ſufficiently impregnated for my purpoſe, I put the liquor into a phial, furniſhed with a proper tube and recipient, ſuch as is repreſented plate II. fig. 8. vol. I. to receive any of the watery part that might be expelled by heat; and applying the flame of a candle, I preſently got from it great plenty of air; which, by every teſt that I could think of applying, appeared to have the very ſame properties with the vitriolic acid air, of which an account was given in the firſt ſection of this volume.

The air thus expelled from this acid liquor was abſorbed by water, without any cruſt on the ſurface of it.

When alkaline air was admitted to it, the ſides of the veſſel were tinged with the orange-colour mentioned above, which vaniſhed in about an hour after it had been expoſed to the open air.

This air had no effect upon *ſalt-petre*; a piece of which continued in it about a fortnight; nor yet upon *brimſtone*, *alum*, or *ſal-ammoniac*.

Liver of ſulphur abſorbed it, without undergoing any ſenſible change.

This air extinguiſhed a candle, without any particular colour of the flame.

Camphor was diſſolved in this air, exactly as it is in vitriolic acid air.

In theſe properties this acid air will be found, by compariſon, to agree with the vitriolic acid air; as alſo in the two following, which, as far as I know, are peculiar to this ſpecies of air.

Phlogiſton, as I have obſerved, is contained in vitriolic acid air, and in ſuch a manner as to be communicated by it to the common air with which it is mixed, and thereby to phlogiſticate or injure it. And an equal quantity of this acid air, and common air, having been mixed, and left together twenty-four hours, the common air appeared to be ſo far injured, that two meaſures of it, and one of nitrous air, occupied the ſpace of ſomething more than two meaſures.

The *electric ſpark* has a very remarkable effect upon the vitriolic acid air, or rather upon the glaſs-tube in which the experiment is made, as will be particularly noted hereafter; for a ſingle exploſion covers all the inſide ſurface with a deep brown or black matter, and the glaſs grows more opake every ſtroke. This very ſingular and ſtriking effect has the electric ſpark taken in the air expelled from this acid liquor.

After I had made this experiment, I had no doubt, but that theſe two kinds of air, viz. the *vitriolic acid* and the *fluor acid*, are, in reality, the ſame. It is poſſible, however, that there may be ſome ſmall difference between them, in conſequence of the air from this acid liquor ſtill containing ſome portion of the earth of the ſpar. I conjecture that it does; becauſe, towards the end of the experiment, when the liquor was made to boil with violence, the inſide of the tube immediately connected with it, was filled with a ſtony matter. It happened twice in the courſe of the above mentioned experiments, that the tube was quite ſtopped up by this means, ſo as to cauſe the exploſion of the phial.

Laſtly, I would obſerve, that the *taſte* of this acid liquor affords a ſtrong preſumption, that the acid which enters into it is the vitriolic; for it has exactly the aſtringency of alum.

That the fluor contains phlogiſton, is evident, from the attempts that I made to procure dephlogiſticated air from it, by means of ſpirit of nitre; for the air that I got from it was always phlogiſticated, and ſometimes even nitrous.

At firſt I made this experiment by putting the materials into a phial with a ground-ſtopple and tube, and applying the heat of a candle only. The air I got in this manner neither affected common air, nor was affected by nitrous air. I then put the ſame apparatus into a crucible; and, with a ſtrong ſand-heat, I got from it about two ounce-meaſures of air, in four portions. The firſt of theſe was exactly like the preceding, being phlogiſticated air; the ſecond made lime-water turbid, and a great part of it was readily abſorbed by water: the third and fourth portions were very ſtrong nitrous air.

This experiment was made with the whitiſh part of this ſpar, which therefore probably contains the leaſt phlogiſton. That phlogiſton which contributes to the *colour* of this foſſil, I found, by the following obſervation, to be of a very volatile nature. When the coloured ſpar is diſſolved in oil of vitriol, the fluor-cruſt, collected in the water, has the ſame colour; but when it is dried near the fire, the colour vaniſhes, and the whole becomes white: yet this white cruſt, heated again in oil of vitriol, contains, as was obſerved before, ſo much phlogiſton, as to convert oil of vitriol into vitriolic acid air.

The air expelled from this acid liquor did not diſſolve the *fluor-cruſt* that was expoſed to it. A quantity of it remained in this ſituation ſeveral days, without affecting it, or being affected by it. I had imagined that it might have been diſſolved by this air, and have converted it into the fluor acid air.

Oil of turpentine abſorbed about ten times its bulk of this air, and became of an orange-colour. After this impregnation it had a pungent acid ſmell, together with its own. I obſerved nothing farther reſpecting it.

Reflecting upon the phoſphoric property of the ſpar, by means of which I had procured this acid air, I thought it was poſſible that its property of enabling oil of vitriol to yield this air, might be common to it with other ſimilar phoſphoric ſubſtances, depending upon that combination of phlogiſton which enables them to imbibe and emit light.

In order to aſcertain this, with reſpect to one other ſubſtance of this kind, I made a quantity of Mr. *Canton's phoſphorus*, and pouring upon it ſome oil of vitriol, I got air that was readily abſorbed by water, and with a cruſt upon its ſurface, exactly like that which is procured from the fluor, only not in ſo great a quantity. The effervеſcence between this ſubſtance and the oil of vitriol was very great, and alſo the heat occaſioned by it; and the vapour eſcaping into the common air, was white and denſe, much like the vapour of the fluor acid.

I ſhall conclude this ſection with obſerving, that the oil of vitriol in which the fluor is diſſolved, becomes thick, like ice, exactly like the oil of vitriol in which quicklime has been boiled, as will be particularly noticed hereafter.

SECTION XII.

Experiments and Obſervations relating to FIXED AIR.

Fixed air was the firſt ſpecies of air that was diſcovered, diſtinct from common air; much has been done towards the inveſtigation of its properties, and ſeveral capital uſes have been made of it. There is ſtill, however, great difference of opinion among philoſophers concerning it; and, in a variety of reſpects, much remains to be done towards compleating our knowledge of it, and eſpecially of its relation to the other acids with which we are acquainted. I cannot ſay that I have, of late, given much attention to this kind of air; but ſeveral things have occurred to me in the courſe of the experiments already recited, and others, which tend to throw a little light upon the ſubject; and a few more obſervations and experiments, not connected with them, I have reſerved for this ſeparate ſection.

Having been informed by a correſpondent in Italy, that air, expelled from lime-ſtone, by means of heat, would not acidulate water; from which he concluded that its acidity, and even its ſubſtance, was derived from the oil of vitriol employed in the production of it: I filled a tall glaſs-veſſel, repreſented fig. *d*, with powdered chalk, and with a ſtrong ſand-heat, expelled from it a conſiderable quantity of air, which appeared to me to be abſorbed by water, exactly like fixed air; the uſual proportion only remaining unabſorbed. It alſo precipitated lime in lime-water; ſo that without impregnating water with it, ſo as to taſte of it, I entertained no doubt of its being genuine fixed air, and having all the properties of the air that is expelled from chalk by oil of vitriol. Mr. Bewley, as the reader will ſee in my Appendix, alſo found that fixed air, procured by means of heat only, changed the blue colour of water (tinged with the juice of turnſole) to red.

Air from wood and charcoal, alſo, is undoubtedly fixed air, though no acid be employed to expel it, and it be mixed with inflammable air. I received air, expelled by heat, from two ounce-meaſures of charcoal in a tall glaſs-veſſel, fig. *d*, in three parts, each containing about a pint; and obſerved, that in every part of the proceſs the air made lime-water turbid. But there was more fixed air in the firſt portion than afterwards; for about one-fourth only of the firſt portion remained unabſorbed by water; but of the ſecond and third portions one-half nearly was unabſorbed. The reſiduum was inflammable.

When heat can expel no more fixed air from charcoal, it ſhould ſeem that ſpirit of nitre (if this acid itſelf be not converted into fixed air) can extract more from it. For when I diſſolved, in ſpirit of nitre, ſome pieces of charcoal, which had been made with the ſtrongeſt heat of a ſmith's fire, long continued, ſo that no more air could be expelled from them by that means; part of it was evidently fixed air, as appeared by its precipitating lime in lime-water.

There are few ſubſtances in nature that do not contain fixed air, diſcoverable either by heat, or by ſome ſtronger acid. In general, acids will detect fixed air more readily than heat; but this is not the caſe with reſpect to *clay*, except when it is ſtrongly heated in ſpirit of nitre; for it makes no effervеſcence upon being mixed with any of the acids: but a degree of heat ſufficient to bake the clay, evidently expels fixed air from it. In order to aſcertain this fact, I filled a gun-barrel with tobacco-pipe clay, and, putting it into the fire, I received the air that came from it, in ſeveral portions; but the whole was not more than about five times the bulk of the clay. The firſt produce was inflammable; but afterwards the air was fixed, precipitating lime in lime-water, and being readily abſorbed by water. I never met with purer fixed air; but I had no ſuſpicion of this at the time that I procured dephlogiſticated air, mixed with fixed air, from clay made into a paſte with ſpirit of nitre.

It might be queſtioned, whether the fixed air contained in our aliments, can be conveyed by the courſe of circulation into the blood, and by that means impregnate the urine. I have found, however, that it may do it; having more than once expelled from a quantity of freſh-made urine, by means of heat, about one-fifth of its bulk of pure fixed air, as appeared by its precipitating lime in lime-water, and being almoſt wholly abſorbed by water; and yet a very good air-pump did not diſcover that it contained any air at all.

It muſt be obſerved, however, that it required ſeveral hours to expel this air by heat; and after the proceſs, there was a conſiderable whitiſh ſediment at the bottom of the veſſel. This was, probably, ſome calcareous matter with which the fixed air had been united; and by this fixed air, the calcareous matter, which would otherwiſe have formed a ſtone or gravel, may have been held in ſolution; and therefore, drinking water impregnated with fixed air, may, by impregnating the urine, enable it to diſſolve calcareous matters better than it would otherwiſe have done, and may therefore be a means of preventing or diſſolving the ſtone in the bladder, agreeable to the propoſal of my friend Dr. Percival; for which ſee the Appendix.

That fixed air is always contained in common air, is evident from many obſervations, and eſpecially from the precipitation of it by means of nitrous air, the electric ſpark, and other phlogiſtic proceſſes. It is likewiſe contained in the pureſt dephlogiſticated air, as appears by mixing nitrous air with it in lime-water, which is thereby rendered ſlightly turbid. It has alſo been ſeen, that when dephlogiſticated air is firſt procured, by any proceſs whatever, there is always a conſiderable quantity of fixed air mixed with it. There is the leaſt when it is got from *mercurius calcinatus per ſe*; but I have always found ſome when the air was expelled from this ſubſtance by a burning lens, either in quickſilver, or *in vacuo*.

In the former volume I have ſaid, that when nitrous air is mixed with common air that had been injured by ſome phlogiſtic proceſs, and reſtored by agitation in water, there was no precipitation of fixed air; but this muſt have been a miſtake: for I have ſince repeated that experiment with the greateſt care, and find a contrary reſult; and I have uſed every precaution that I could think of, in order to guard againſt a miſtake in the proceſs; particularly, leſt the air upon which I was operating ſhould receive a mixture of any other kind of air from the water in which it was agitated, I previouſly boiled the water for ſeveral hours, in order to expel all its own air from it.

Having done this, I obſerved that, immediately after the reſtoration of the noxious air by agitation in this water, it always made lime-water ſlightly turbid; but this was not the caſe, after paſſing two or three times through the lime-water. When it was, by this means, intirely purged of fixed air, I admitted nitrous air to it in lime-water, and there was a very evident precipitation of lime, quite as much as when nitrous air is mixed with common air that has not been injured at all.

It is not eaſy to ſay whence this fixed air could come. If all the fixed air had been diſcharged

charged by the firſt phlogiſtic proceſs, that which appeared in the ſecond muſt either have come from the water, though it had been boiled, which I do not think probable, or from the nitrous air, which, though it be inexplicable, is perhaps leſs improbable upon the whole.

Mr. Cavendiſh obſerved, that a certain portion of fixed air is no more liable to be abſorbed by water than common air. This, he ſtates at about one-ſixtieth part of the whole. I had the curioſity to try, whether, if I ſaturated a quantity of water with fixed air, and expelled it again by heat, that very air which had actually been in the water, would not be wholly imbibed by freſh water; and whether I could not, by this means, get a purer kind of fixed air than that which is immediately procured by means of chalk and oil of vitriol. This experiment I made twice, with all the care that I could apply, and found, in both the caſes, that even the fixed air which had been in the water, contained as large a portion of that which would not be imbibed by water again, as the air which had been immediately diſlodged from chalk by oil of vitriol.

In order to be more ſure of this fact, I was more eſpecially careful, the ſecond time that I made the experiment, to uſe every precaution that I could think of, in order to prevent any error in the concluſion. For this purpoſe, I took rain-water, and boiled it about two hours, in order to get it perfectly free from air; and I began to impregnate it with fixed air a long time before it was cold, and therefore before it could have imbibed any common air; and, in order to expel the air from it, I put it into a phial, which I plunged in a veſſel of water ſet on the fire to boil, taking care that both the phial containing the impregnated water, and the glaſs-tube through which the air was to be tranſmitted, were completely filled with the water, and no viſible particle of common air lodged on the ſurface of it. I alſo received the expelled air in water, which contained very little air of any kind, leſt the very ſmall degree of agitation which I made uſe of, in order to make the water re-imbibe the air, ſhould diſengage any air from it. Alſo, that leſs agitation, and leſs time, might be ſufficient, I chiefly made uſe of lime-water for this purpoſe. But notwithſtanding all theſe precautions, I found a very conſiderable reſiduum of air, not leſs than Mr. Cavendiſh had ſtated, that water would not imbibe.

At a time when this reſiduum of fixed air hardly gave the leaſt ſenſible whiteneſs to lime-water, I examined the ſtate of it, and found, by the teſt of nitrous air, that it was very little worſe than common air; two meaſures of this air, and one of nitrous air, occupying the ſpace of two meaſures only.

This fact will be thought a pretty remarkable one; and I can give no ſatisfactory account of it, unleſs the following ſhould be deemed to be ſo. Fixed air, phlogiſticated with iron-filings and brimſtone, or with the electric ſpark, I have diſcovered, vol. I. p. 42. to become, in a much greater proportion than uſual, immiſcible with water; and I therefore concluded that this acid air (for ſuch fixed air evidently is) by combining with phlogiſton, comes to be a kind of air ſimilar to common air. If this be a juſt account of the former experiment, the fixed air in this caſe muſt get phlogiſton from the water with which it was combined, and thereby become, in part, immiſcible with water. That water, even the pureſt, does contain phlogiſton, is, I think, evident from the experiments which ſhew that air is injured by much agitation in it. Or, if an earthy matter, and not phlogiſton, be neceſſary to the conſtitution of reſpirable air, as, I think, appears from my experiments on dephlogiſticated air, there may be enough of it held in ſolution in the pureſt water, and which it may impart to the fixed air combined with it.

All water, which has been any time expoſed to the atmoſphere, contains more or leſs air, part of which is, I believe, always fixed air. This abounds ſo much in ſome mineral waters, that their peculiar virtues are certainly owing to this ingredient in their compoſition. This conſideration has led ſome perſons to aſcribe the virtues of other mineral waters to this principle, though they contain it in ſo very ſmall a proportion, as to make that opinion very improbable. Some, for inſtance, have thought that the virtues of the *Bath water* were owing, in a great meaſure, to the fixed air it contains; and living at no great diſtance from that celebrated ſpring, I thought I ſhould incur a juſt cenſure, if I did not endeavour to aſcertain what kind of air is contained in that water, and in what proportion. Accordingly, I made an excurſion as far as Bath, chiefly with that view, and made the following experiments, which, having no apparatus of my own along with me, I was enabled to perform by the friendly zeal and ingenuity of Mr. Painter; Dr. Guſthart, Dr. Falconer, and Dr. Watſon favouring me with their preſence.

In order to aſcertain what proportion of air is contained in the water, in the ſtate in which it is drank, I filled a pint-phial with water, hot from the pump, and expelled the air from it, by boiling it about four hours, receiving the produce in quickſilver. This air was about $\frac{1}{30}$ of the bulk of the water, and about one-half of it was fixed air, precipitating lime in lime-water, and being readily abſorbed by water. The reſiduum appeared, by the teſt of nitrous air, to be rather better than air in which a candle had burned out.

The quantity of fixed air that appears, by this experiment, to be contained in the Bath-waters is ſo very ſmall, that I think it very improbable that their virtues ſhould be at all owing to it. Few ſpring-waters, I believe, contain much leſs fixed air, and many I know, which have no medicinal virtue at all, contain more. The pump-water belonging to the houſe in which I now live, contains about $\frac{1}{14}$ of its bulk of fixed air; and it may be ſeen in my former volume, p. 160, that my pump-water at Leeds contained about $\frac{1}{50}$ of its bulk of air, of the very ſame compoſition as the air of the Bath-waters, viz. half of it fixed air, and half common air a little phlogiſticated, ſo as to be in about the ſame ſtate as air in which a candle had burned out.

Beſides, the length of time which the Bath-waters, and indeed moſt other ſpring-waters, require to expel the air by means of heat, ſhews that the air expelled from them, was not contained in them in that ſtate in which it is contained in waters properly impregnated with fixed air, out of which it may always be expelled by the heat of boiling water in leſs than an hour. In fact, the fixed air is not united to the *water*, but to ſome *calcareous matter* in the water, out of which the air is expelled with much more difficulty. Accordingly, Dr. Falconer informs me, that there is a depoſit made by this water, after long boiling; if ſo, it may be preſumed, that theſe waters do not ſo properly contain fixed air, as a calcareous earth; which, though it contain fixed air, may not part with it in the ſtomach, unleſs it meet with ſome acid to decompoſe it.

Beſides the air contained in the Bath-water, there is a conſiderable quantity of air continually bubbling up from almoſt every part of the ſoil, through the water in the bath. When I was about to examine this air, Dr. Falconer informed me, that it had been done already by Dr. Nooth, and that an account of his experiments was inſerted in the ſecond volume of his treatiſe on the Bath-waters. The paragraph relating to it is as follows:

“ At the place where the ſprings riſe in the
“ baths, numerous bubbles of air are obſerved
“ to

" to aſcend along with them. A quantity
" of air of this kind was collected at the King's
" Bath, by inverting a glaſs, and holding it
" over the bubbles as they roſe, and then
" conveying it into an inverted bottle, which,
" when full, was carefully corked up, and
" carried away. The air thus obtained an-
" ſwered in every reſpect to fixible air, preci-
" pitating lime in lime-water, and having
" every other quality which that ſubſtance
" poſſeſſes."

Being informed of this, I thought it unneceſſary to repeat the experiment; but finding, upon inquiry, that Dr. Nooth had not examined what proportion the reſiduum of the fixed air bore to the whole, or of what quality that reſiduum was, though he ſpeaks of the whole as containing *every quality that fixed air poſſeſſes*, I thought it would not be amiſs, as I was upon the ſpot, to make the trial myſelf. Accordingly, I took about a pint of that air, in nearly the ſame manner that Dr. Nooth had done, and found, upon examination, that only about $\frac{1}{20}$ of its bulk was fixed air, precipitating lime in lime-water, and being readily abſorbed by water. The reſt extinguiſhed a candle, and was ſo far phlogiſticated, that two meaſures of it, and one of nitrous air, occupied the ſpace of $2\frac{19}{20}$ of a meaſure; that is, it was almoſt perfectly noxious.

Had I had more leiſure, and a better apparatus, the experiments might have been made with more accuracy; but I do not think that, whenever they are repeated, they will be found to be materially wide of the truth, though it is poſſible that the ſtate of the air in the water, and eſpecially that which riſes through the water, may be ſubject to variation. The meaſures were only eſtimated by the eye; but then all who were preſent agreed very nearly in the ſame eſtimation.

Being in Germany in the ſummer of the year 1774, we happened to paſs by the famous ſpring of Seltzer-water, near Schwallbach, and alſo another very hot ſpring near the road from that place to Mentz. Through both theſe ſprings there was a bubbling of air, exactly ſimilar to that in the Bath-waters; but I had not time, or convenience, for making the ſame experiments upon them, and therefore contented myſelf with finding that the air of both of them extinguiſhed a candle.

It is well known that all fermented liquors, that are not quite flat or vapid, contain fixed air; and I had the curioſity to try, what proportion

portion of this air is contained in different kinds of wine, and in wines in different ſtates. For this purpoſe, I took one of the phials with a ground-ſtopple and tube, repreſented fig. *e*, containing 1 ½ of an ounce-meaſure, and filling it accurately with each ſpecies of wine, I plunged it into a veſſel of water, which was ſet on the fire to boil, receiving the air in quickſilver. The air that I got from all kinds of fermented liquors was pure fixed air; but, except champaigne and cyder, it was in much leſs quantity than I expected; the reſults being as follows.

The quantity of air contained in

Madeira, was -	$\frac{1}{100}$	of an ounce-meaſure.
Port of 6 years old -	$\frac{1}{48}$	————————
Hock of 5 years -	$\frac{1}{24}$	————————
Barrelled Claret -	$\frac{1}{12}$	————————
Tokay of 16 years -	$\frac{1}{20}$	————————
Champaigne of 2 years	2	————————
Bottled Cyder of 12 years	3¼	————————

Some champaigne ſparkles much in conſequence of containing much air; but there is a kind of champaigne which does not ſparkle, and contains very little air. The difference, as I was informed, when I made inquiry concerning it, in that part of France where the wine is made, is owing to this; that when they wiſh to have the wine ſparkle, they check the fermentation as much as poſſible at the time that the wine is made; ſo that the fermentation going on gradually, the fixed air produced by it is abſorbed by the liquor: whereas, when they do not chuſe to have it ſparkle, they let it ferment freely, like any other kind of wine.

In other caſes, therefore, where fermented liquors contain much air, as in moſt kinds of malt-liquor, cyder, and our Engliſh made-wines, I take it for granted, that the fermentation is either purpoſely checked, or that the liquor is of ſuch a nature, that the fermentation will neceſſarily continue a long time, after it is put into the caſk or bottle.

I once found that a quantity of port-wine contained its own bulk of fixed air; but I now imagine that the wine was not genuine, but muſt have been made chiefly of cyder. Perhaps this may not be a bad method of diſtinguiſhing genuine foreign wines from compoſitions made of cyder.

SECTION XIII.

Miſcellaneous Obſervations.

I.

I have mentioned a fact, which ſhews that *chalk* retains fixed air very obſtinately; ſo that neither the ſolar rays, nor the ſtrongeſt heat of a ſmith's fire, continued for a long time, can expel the whole quantity that it contains. I have alſo found, that a ſmall quantity of fixed air was contained in the beſt *quicklime* that I could procure; ſince ſtrongly concentrated acids would ſtill expel a ſmall quantity from it. I mention this, chiefly, for the ſake of an obſervation which may not be new, but which, if it be new, may be of ſome uſe, viz. that when I had heated ſome pieces of quicklime in oil of vitriol, in order to extract from it all the air that I poſſibly could, the next day I found the oil of vitriol ſolid and tranſparent, exactly reſembling a thick jelly; but it became fluid again with the heat of my hand. This may, probably, be a good and expeditious method of concentrating this acid, the quicklime abſorbing its water.

II.

I made a beginning of a courſe of experiments, which, I think, may be purſued to conſiderable advantage, on the ſtate of the air which is contained in the *bladders of fiſhes*. It is commonly ſuppoſed, that theſe bladders are of no other uſe to the fiſhes than to aſſiſt them in riſing or ſinking in the water: but I have ſome doubt about this hypotheſis; at leaſt they may have ſome other uſe. Some fiſhes, I believe, are not furniſhed with theſe bladders. When they are taken out of the fiſh, the air cannot be got from them by preſſure, but I was always obliged to burſt or cut them; and yet that the air does change in theſe bladders, is, I think, pretty evident, from my having found it in different ſtates.

The firſt time that it occurred to me to examine the air contained in theſe bladders, I found it, in a great number of them, to be perfectly noxious, not being at all affected by nitrous air. This was on the 31ſt of May, 1774. But at another time, viz. the 30th of March following, I found air that I had preſſed from the bladders of the ſame kind of fiſhes, viz. roaches, not to be quite noxious, being affected by nitrous air, though not to a great degree. I have not purſued theſe experiments any farther; but I ſhould think that it might not be difficult, by diverſifying them properly, to make ſome diſcoveries concerning the animal œconomy of fiſhes, and the uſe of air to them.

III.

That excellent anatomiſt, Mr. John Hunter, told me, that fiſhes would not live in water impregnated with fixed air. I repeated the experiment, and found that ſmall fiſhes would not live in this kind of water more than a few minutes. At the ſame time I had the curioſity to try how they would be affected by water impregnated with *nitrous air*, and obſerved that they were affected in the ſame manner, but much more violently; being thrown into the greateſt agitation the moment they were put into it, and moving about with the greateſt rapidity, till they became languid and died. A courſe of experiments of this kind, joined with the other, would, I think, be very promiſing to a perſon who had an opportunity of making them to advantage.

IV.

In ſome chymical proceſſes, volatile alkali diſſolves copper. This I alſo have obſerved in my account of the experiments in which I put ſome pieces of volatile alkaline ſalt to a quantity of common air, at the time that I introduced nitrous air to it, vol. I. p. 213. For, if the alkaline ſalt be ſupported by copper-wire, it preſently becomes blue, and is ſoon corroded. I therefore thought that pieces of copper, expoſed to pure *alkaline air*, would have been affected in the ſame manner; but I did not find this to be the caſe. A number of pieces of copper-wire remained a whole night in alkaline air without ſenſibly affecting it, or being affected by it. That the alkaline air was pure, appeared by its being wholly abſorbed by water afterwards.

V.

I had ſome expectation that alkaline air might be expelled from cauſtic *fixed alkali*, eſpecially as it is known that the fixed and volatile alkalis differ only in their combinations; but I was diſappointed in my expectations. Having procured a quantity of cauſtic alkali from Mr. Lane, who is known to prepare it with particular accuracy, I treated it in the ſame manner as I had done the ſpirit of ſalt, and found that the vapour expelled from it conſiſted of nothing but water, being immediately condenſed when it came to the cold quickſilver.

VI.

I have obſerved before, that though the marine acid air does not become inflammable air by means of liver of ſulphur, as it does by means of many other ſubſtances that contain phlogiſton; yet that it does form a permanent kind of air, which appeared to be phlogiſticated, by extinguiſhing a candle, though the quantity which I then produced was ſo ſmall, that I did not pretend to form an accurate judgment concerning it. I have, ſince that, made another experiment of the ſame kind, rather more deciſive than the former. I put ſeveral pieces of liver of ſulphur to a quantity of marine acid air; when I obſerved that it preſently began to be abſorbed, and it continued in that ſtate till one-half of the whole had diſappeared. By this time the liver of ſulphur, which had been of a greeniſh or yellowiſh colour, became white. Afterwards more liver of ſulphur abſorbed more of this air; but after two days the pieces began to diſſolve, and at length they became one liquid maſs, the air ſtill diminiſhing very gradually. In this ſtate I admitted water to the air; but by this very little more of it was abſorbed; and that which remained was about one-fourth of the original quantity, and extinguiſhed a candle. The whole proceſs was three days. After this air had ſtood a week a week in water, and had been a little agitated in it, it was a litle diminiſhed by nitrous air.

VI.

I have mentioned a courſe of curious experiments on the mixing of *ether* with ſeveral kinds of air, the conſequence of which was, that the quantity of each of them was almoſt inſtantly doubled by a ſingle drop of that fluid; but that afterwards, water abſorbed the ether, leaving the air poſſeſſed of all its peculiar properties. Thoſe experiments were made with vitriolic ether. Having, ſince, procured a quantity of *nitrous ether*, made by Mr. Godfrey, I had the curioſity to try whether this would produce the ſame effect; but I found that it increaſed common air only about one-ſixth of its bulk. After this mixture had continued two days and a night, water abſorbed the ether, and left the common air exactly, or very nearly, the ſame as before, judging by the teſt of nitrous air.

VII.

In my experiments to extract air from freſh-made red lead, by mixing it with ſpirit of nitre, I had the curioſity to try what would be the effect of mixing it in the ſame manner with a volatile alkaline water; but no air was produced from it, neither did the red lead acquire any additional weight from the mixture.

VIII.

Conſidering the very different properties of the different kinds of air with which I have been converſant, it was impoſſible not to think of the probability of their having different *refractive powers*, and of ſome method of aſcertaining this circumſtance. Accordingly I intended to have done ſomething of this kind before the publication of my former volume on this ſubject; but I was prevented by an unexpected delay in the conſtruction of the apparatus which I had contrived for that purpoſe. I have ſince completed my apparatus, and have made the trials which I then propoſed; but I am ſorry to inform my readers, that they have been without any ſucceſs.

For this purpoſe, I procured a priſm, conſiſting of three plates of glaſs, faſtened together by cement, the cavity being large enough to contain about a quarter of a pint. This priſm I fixed upon a ſtand, at the diſtance of ten feet from a window, in which I had a ſmall apparatus, contrived to throw a beam of the ſun's rays into the room. This beam was received by a board, furniſhed with a piece of braſs-work, containing ſeveral ſmall holes, through any of which I could tranſmit a beam of light upon the priſm, which was placed, in a vertical poſition, cloſe behind it; and the wall on which the image of the ſun was received was twenty feet from the priſm.

With this apparatus, which I thought promiſing enough, I proceeded to try the refractive powers of nitrous and inflammable air; but I could perceive no difference in the place of the image, whether the beam of light was tranſmitted through the priſm, carefully filled with either of theſe kinds of air, or not through it; allowance being made for a ſmall degree of refraction occaſioned by a want of perfect parallelíſm in the plates of the priſm. The reſult was the very ſame, whether it contained common air, or either of the two kinds above mentioned.

Having had ſo little ſucceſs with theſe two very different kinds of air, I thought it would be in vain to try any of the other kinds; and therefore, for the preſent, have deſiſted from my purſuit; but I am not without a deſign to reſume it with a different kind of apparatus, if I be ſo happy as to ſucceed in the conſtruction of it.

IX.

The facility with which the nitrous acid forms air of various kinds is very remarkable; eſpecially when compared with the two other mineral acids, which enter into the compoſition of few kinds of air in compariſon with this. I was in hopes that, by ſubſtituting thoſe acids in the place of the nitrous, in the experiments which produced the dephlogiſticated, and other kinds of air, I ſhould, at leaſt, have got *ſome* kind of air; but I got none. I have mentioned my having tried this with red lead. I alſo made the ſame attempt with the marine acid, and dried fleſh, from which I got the peculiar kind of air deſcribed ſect viii.; but this produced nothing but the marine acid air, in quickſilver, and nothing at all in water; the acid air being abſorbed by it as faſt as it was generated. Trying a piece of *beef* with the ſame apparatus, without any acid, it yielded, by means of a pretty ſtrong heat, from the flame of a candle, inflammable air, as in the ſimilar experiment with a gun-barrel, mentioned in my former publication.

X.

I have obſerved, in the firſt volume of this work, that when I had put a piece of *ſalt-petre* to a quantity of marine acid air, it was preſently diſſolved, emitting a white fume; but that no air, that I could examine, remained, the quantity of it being ſo ſmall. I have ſince repeated the experiment; but the reſult was nothing more than might have been predicted; for the nitrous acid, diſlodged from its baſe by the marine, had diſſolved ſome of the quickſilver, and formed nitrous air, occupying one-half of the whole ſpace that had been filled by the marine acid air.

Marine acid air affects *borax*, in the very ſame manner in which alkaline air affects alum, rendering it whitiſh.

XI.

At the time of my former publication, I had found that taking the *electric ſpark* in given quantities of ſeveral kinds of air, had a very remarkable effect upon them, that it diminiſhed common air, and made it noxious, making it depoſit its fixed air, exactly like any phlogiſtic proceſs; from whence I concluded that the electric matter either is, or contains phlogiſton. It has alſo the ſame effect as a phlogiſtic proceſs on nitrous air, diminiſhing it very much, and depriving it of its property of diminiſhing common air I have ſince repeated this experiment on ſome other kinds of air which cannot be confined by water, and I find the reſults to be no leſs remarkable, though I have not given ſo much attention to them as to be able to explain them. The facts were as follows.

Having made about fifty electric exploſions of a common jar, in a ſmall quantity of the *marine acid air*, confined in a glaſs-ſyphon by quickſilver, I obſerved that it was a little diminiſhed, and that a ſmall part of the inſide of the glaſs, next to the quickſilver, was tinged white. Water admitted to this air abſorbed ſo much of it, that no experiment could be made on the remainder.

I made the ſame experiment on the *vitriolic acid air*; when preſently the inſide of the glaſs through which the exploſion paſſed was uniformly covered with a blackiſh matter, ſo that nothing could be ſeen through it, and the air ſeemed to be rather increaſed than diminiſhed. Water being admitted to it, left ſo little of it unabſorbed, that it could be no more examined than that which remained in the preceding experiment. Part of the blackiſh matter was waſhed off by the water.

I took the electric exploſion in a ſmall quantity of *alkaline air*, in the ſame manner as in the two preceding experiments, and obſerved, that every ſtroke added conſiderably to the quantity of air; and when water was admitted to it, juſt ſo much remained unabſorbed as had been added by the exploſions. I then took about an hundred exploſions of the ſame jar, in a larger quantity of alkaline air; after which, ſo much of it remained unabſorbed by water, that I could examine it with the greateſt certainty. It neither affected common air, nor was affected by nitrous air, and was as ſtrongly inflammable as any air that I had ever procured.

Theſe experiments appear to me to furniſh matter for much ſpeculation, and farther experimental inquiry. Till this be done, all *conjecture* concerning them muſt be very much at random. I therefore defer making any at preſent.

SECTION XIV.

Experiments and Obſervations on CHARCOAL, *firſt publiſhed in the* Philoſophical Tranſactions, vol. LX. p. 211.

Among the original experiments, publiſhed in the Hiſtory of Electricity, was an account of the conducting power of charcoal. This ſubſtance had been conſidered by electricians, in no other light than that of more perfectly baked wood, which is known to be no conductor of electricity. I have even heard of attempts being made to excite it; and though thoſe attempts were ineffectual, the failure of ſucceſs was attributed to other cauſes than that of charcoal being no electric ſubſtance; ſo fixed was the perſuaſion, that water and metals were the only conducting ſubſtances in nature. The conſideration of the chymical properties of charcoal, which are, in many reſpects, remarkably different from thoſe of the wood from which it is made, might have led philoſophers to ſuſpect, that ſince, after its being reduced to a coal, it was become quite *another thing* from what it was before, it might poſſibly differ from wood in this propetty; but this conſideration had not been ſufficiently attended to.

In the account of my former experiments on charcoal, I obſerved, that there were very great differences in the conducting power of charcoal, and particularly of wood-charcoal, though I could not determine on what circumſtances in the preparation, &c. thoſe differences depended. I therefore expreſſed a wiſh, that ſome perſon, who had conveniencies for making chymical experiments, would proſecute the inquiry, as one that promiſed, not only to aſcertain the cauſe of the conducting power of charcoal, but perhaps of *conducting power univerſally*. Not hearing that any chymiſt or electrician has attended to this buſineſs, I have, at length, reſumed the ſubject, though not with every advantage that I could have wiſhed. I have, in a great meaſure, however, ſucceeded in the principal object of my inquiry; and I ſhall now lay before this ſociety the reſult of my experiments and obſervations.

I ſhall begin with correcting a miſtake I lay under at the time that I made the former experiments. Having been informed by perſons, who attend the making of *pit-charcoal*, that it was conſiderably increaſed in bulk after the proceſs; I imagined that all other ſubſtances received an increaſe of bulk, when they were reduced to a coal; but the firſt experiments that I made, convinced me of my miſtake. All vegetable ſubſtances are conſiderably contracted in all their dimenſions, by the proceſs of coaling, and the more perfect this proceſs is (that is, as will be explained hereafter, the greater is the heat that is applied in the courſe of it) the greater is the diminution. I have even reduced pieces of wood to little more than one-fourth of their original length and breadth, in a common fire, by the uſe of a pair of hand-bellows only. And this was the caſe equally with wood of the firmeſt texture, as ebony; that of a middle texture, as oak; and that of the looſeſt, as fir, &c.

As moiſture (and, I believe, ſmall degrees of heat or cold) affects wood much more ſenſibly *acroſs* the fibres than *along* them, it might have been ſuppoſed, that when wood was reduced to a coal, by the application of a greater degree of heat, the ſame rule would have been obſerved; but I found very little difference in this reſpect. To aſcertain this circumſtance, I took from the ſame board, two pieces, each $2\frac{1}{2}$ inches in length. In one of them, the fibres were divided, in the other they were not; and after coaling them thoroughly together, in the ſame crucible, I found that the former meaſured 2.05 inches, and the latter 2.15. Their conducting power could not be diſtinguiſhed.

A more particular account of the degree, in which wood is ſhortened in coaling, will be ſeen afterwards, when the variations in this reſpect are compared with the variations in the power of conducting electricity.

To my great ſurprize, I found *animal ſubſtances* not reduced in their dimenſions by the proceſs of coaling. This, at leaſt, was the caſe with ſome pieces of *ivory*, ſeveral inches in length, and a piece of *bone*. They bore a very intenſe heat for many hours, and came out of the crucible conſiderably diminiſhed in weight, but hardly ſo much as diſtorted in their ſhape, as is remarkably the caſe with wood, and, I believe, all vegetable ſubſtances.

In examining mineral ſubſtances, I found that my information, mentioned above, was juſt. Coals are very much enlarged in their dimenſions by charring; but the experiment muſt be made with great care, to judge of this circumſtance; for, unleſs the operation be very ſlow, the coal will retain nothing of its former ſhape, having been made, in ſome meaſure, fluid by the heat. The inſide of all pieces of

pit-

pit-charcoal is full of cavities, and there is generally a very large one in the center of every piece; ſo that the dilatation is nothing like the extenſion of fibres; but is produced by the elaſticity of the new-formed vapour, in forcing its way out, while the ſubſtance is ſoft.

With reſpect to the main object of my inquiry, I preſently ſatisfied myſelf, that the conducting power of charcoal depends upon no other circumſtance than the *degree of heat*, that is applied in the proceſs of making it. I had not ſuſpected this; but numberleſs experiments clearly proved it. Taking an iron-pot, filled with ſand, and putting into it pieces of wood, cut out of the ſame plank, marking them, and carefully noting their places in the pot, I always found that thoſe pieces came out the beſt conductors, that had been expoſed to the greateſt heat. The reſult was the ſame when I made coals of bits of wood, placed one above another, in a gun-barrel, one end of which was made red-hot, and the reſt gradually cooler and cooler.

Taking pieces of charcoal that conducted very imperfectly, or not at all, I never failed to give them the ſtrongeſt conducting power, by repeating the proceſs of coaling, either in

a crucible,

a crucible, or in a gun-barrel, covered with ſand, and kept in an intenſe heat.

I could not find that the mere continuance of the ſame degree of heat had any effect with reſpect to the conducting power of charcoal.

Mr. Macquer, and other chymiſts, define charcoal to be *wood burned, without being ſuffered to flame*; but, with reſpect to its conducting power, and, I make no doubt, with reſpect to all its other eſſential properties alſo, it makes no difference whether it flame or not. I have coaled pieces of wood, both in gun-barrels, and in crucibles, ſlightly covered with ſand, and have let the inflammable vapour that exhaled from them take fire, at various diſtances from the ſubſtances; and I have alſo put pieces of wood in an open fire, and urged the heat applied to them, with a pair of bellows; and in all theſe caſes have found the charcoal equally good. In the laſt method, indeed, very little of the ſubſtance is preſerved; but the little that doth remain, after it hath ceaſed to flame, whether it be quenched immediately, or not, conducts as well as any charcoal whatever. But one can hardly be ſure that the ſame degree of heat is given to every part of a piece of wood, except it be expoſed to it for ſome time; and in an open fire, urged with a pair of bellows, the wood waſtes as faſt as it is red-hot, before the center of it is much affected with the heat.

When once any degree of conducting power is given to a piece of charcoal, I never found that it was afterwards leſſened. A partial conſuming of it in an open fire doth not affect the remainder, as I obſerved in the account of my former experiments.

I had imagined, that the *ſolidity* of ſubſtances converted into charcoal, would have had a very conſiderable effect on their conducting power afterwards; but the conjecture was not confirmed by experiment. Coals made of the lighteſt woods conducted, as far as I could perceive, as well as thoſe that were made from the moſt ſolid, if they had been expoſed to the ſame degree of heat in the proceſs. Fine ſhavings of fir, the fine coats of an onion, the lighteſt ſoot, and every other vegetable ſubſtance that I tried, conducted equally with coals made of oak or ebony.

I had imagined, alſo, that the moment a piece of wood was become black with heat, it was, to all intents and purpoſes, a real charcoal; and, along with the other properties of charcoal, would conduct electricity, more or leſs; but I found, by coaling ſeveral pieces very ſlowly, that they would not conduct in the leaſt degree, not only when they were made ſuperficially black, but likewiſe when they were black quite through, and had remained a long time in the heat that made them ſo; ſo that no eye could diſtinguiſh them from the moſt perfect charcoal.

I have ſometimes found charcoal in ſuch a ſtate, that it would aſſiſt the paſſage of an exploſion along its ſurface, when it would not conduct a ſhock any other way.

In order to ſatisfy myſelf in what proportions the *diminution of weight*, the *decreaſe of bulk*, and the *conducting power* of wood and charcoal, correſponded to one another, I took ſeveral pieces from the ſame plank, and having carefully weighed and meaſured them, converted them into coals very ſlowly, and by a gradual increaſe of heat, on an iron plate, held on the fire, turning them conſtantly, to prevent their catching fire. The following were the reſults.

A piece of very old dry oak, weighing 12 grains, and which conducted in the imperfect manner that wood generally does, from the moiſture it contains, was, after the loſs of about one grain, no conductor at all; and it continued the ſame as baked wood, till it was reduced to four grains, when it was black quite through; and even then, no part of it conducted, except one corner, where it had catched fire.

Another piece I carefully weighed, and meaſured ſeveral times in the courſe of the proceſs. At firſt it weighed

Gr.		Length.	Bread.	Thick.
12	when its dimenſions in inches were	2.	.45	.12
At 8	———————————	2.	.4	.12
— 5 .5	———————————	1.91	.4	.12
— 3 .5	———————————	1.8	.35	

It was now become an imperfect conductor. I then urged it with a ſtrong heat, in a crucible, and taking it out, it weighed 1 .75 gr. and meaſured 1 .6 in length, and .3 in the other dimenſions. It was now a perfect conductor; and though I afterwards kept it in a very intenſe heat ſeveral hours, by which it was reduced to one grain in weight, its conducting power was not ſenſibly increaſed; but it was become very brittle, or friable.

It appears from theſe experiments, that theſe pieces of wood were reduced to about one-fourth of their weight before they would conduct at all; though, at the ſame time, they were diminiſhed in length (*i. e.* along the fibres) only one-tenth. The breadth and thickneſs could not be meaſured with ſufficient accuracy in theſe ſmall pieces. To make them perfect conductors, they were reduced to about one-tenth in weight, and one-half in length.

A variety of circumſtances led me to conclude, that the cauſe of *blackneſs*, and of the conducting power in charcoal, is the oil of the plant, made empyreumatic, and burnt to a certain degree. I therefore conclude that theſe properties are ſome way connected with that part of the inflammable principle, otherwiſe called phlogiſton, that is fixed and united to the earth of the plant, when the union is ſtrengthened by an intenſe heat.

The *ſand*, with which I covered the ſubſtances that I converted into coals, and alſo the *pipe-clay* which I ſometimes put over them, contracted a blackneſs like charcoal, and would often conduct pretty well. Sometimes they would conduct a ſhock. This muſt have been owing to the oil they received from the ſubſtances out of which it was expelled by the heat. In the experiment of the gun-barrel filled with pieces of wood, mentioned above, the uppermoſt pieces were not in the leaſt burned.

They could hardly have been hot; yet, having contracted a ſuperficial blackneſs, from the vapour of the oil expelled from the piece below them, they would even conduct a ſhock, though not in the moſt perfect manner.

Sometimes thoſe ſubſtances that had no phlogiſton themſelves, but received it in conſequence of being placed in the neighbourhood of other bodies out of which it was expelled, would not conduct immediately; but would be made to do ſo by being expoſed to a greater heat, which more thoroughly burned the oil with which their pores were filled.

I put a piece of a common *pipe* into a crucible, in which I was burning ſome turpentine (which will be mentioned below) and it came out black quite through, like a pipe in which tobacco has been frequently ſmoked. In this ſtate it would not conduct at all; but putting it into a crucible, covered with ſand, I treated it in the ſame manner as I would have done a piece of wood, in order to coal it, and it came out a very good conductor. Had it been burned in the open fire, the phlogiſton would have eſcaped, and the pipe would have been left white, as at firſt.

Being convinced that the conducting power of charcoal depended upon the oil, or rather the phlogiſton contained in the oil, and on the degree of heat with which it was burned, I took ſeveral methods to give vegetable ſubſtances more of this principle; or at leaſt endeavoured to make them retain more of it than they uſually do, in the proceſs of coaling. But I had no apparent ſucceſs in thoſe experiments.

I began with plunging a piece of old dry oak in oil; and then, pumping the air out of it, let it ſtand *in vacuo* a day and night, in which time it ſeemed to diſcharge a great quantity of air; after which I let the air into the receiver, and thereby forced the oil into its pores. But the coal from this wood was not ſenſibly better than others. The application of heat may, perhaps, expel the phlogiſton in ſuch a manner, that the reſiduum, being fully ſaturated, can retain no more than a certain proportion. I made coals of other pieces of wood, when they were covered with cement; and I alſo coaled ſeveral pieces together, that they might receive phlogiſton from one another; but, in both caſes, without any ſenſible improvement in the quality of the coal.

In order to prevent the eſcape of the phlogiſton belonging to the ſubſtance to be reduced to a coal, I put ſome pieces of wood into a gun-barrel, and corked it as cloſe as I could, at the ſame time covering the cork with cement. In this caſe the rarefaction of the exhaling vapour never failed to drive the cork out; but it muſt have been after a conſiderable reſiſtance to its eſcape. However, I could not perceive any peculiar excellence in the charcoal made in this manner.

I do not, indeed, know any method in which differences in ſubſtances that conduct ſo well as theſe can be accurately tried, at leaſt none that can be applied in this caſe. The charcoal I can make in a common fire, by the uſe of a pair of hand-bellows, I cannot diſtinguiſh, with reſpect to its conducting power, from the moſt perfect metals, gold and ſilver; either by the length of the electric ſpark, the colour of it, or the ſound of the exploſion. I make no doubt but that wood, in the proceſs of coaling, may eaſily have a degree of conducting power communicated to it, exceeding that of lead, iron, or the other more imperfect metals.

We may, perhaps, be guided in our conjectures on this ſubject, by conſidering the *degree of heat* that is neceſſary, either to unite the phlogiſton to its baſe, or to ſeparate them, both in the caſe of wood, and the different metals.

Lead is very eaſily calcined, and it is alſo known to conduct electricity very imperfectly. Iron ſoon turns to ruſt; and its conducting power I found to be very ſmall, in compariſon with that of copper, or the more perfect metals. If, therefore, in making charcoal, a degree of heat be applied greater than is neceſſary to calcine or revivify a metal, we may perhaps conclude, that the conducting power of the charcoal will be ſuperior to that of the metal. As it may be poſſible to give charcoal, when cut off from any communication with the external air, a greater degree of heat than ſilver or gold would bear without being diſſipated in vapour; it may even be poſſible to make charcoal that ſhall conduct electricity better than thoſe moſt perfect metals.

Had there been any phlogiſton in water, I ſhould have concluded, that there had been no conducting power in nature, but in conſequence of ſome union of this principle with ſome baſe. In this, metals and charcoal exactly agree. While they have the phlogiſton, they conduct; when deprived of it, they will not conduct *.

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* Having ſince found, that long agitation in the pureſt water injures air, ſo that a candle will not burn in it afterwards, which is preciſely the effect of all *phlogiſtic proceſſes*, I now conclude that the maxim ſuggeſted in this paragraph is univerſally true. See vol. I. p. 283.

I believe, however, that all vegetable or animal ſubſtances, that contain phlogiſton, may be reduced to a coal; and if the heat applied in the proceſs be ſufficient, that coal will conduct electricity. Fleſh, glue, bones, and other parts of an animal body, make good conducting charcoal.

The only approach, or ſeeming approach, I ever made towards retaining more phlogiſton than uſual, in wood reduced to a coal, was by the *ſlowneſs of the proceſs*. For I always found, that if the heat was applied very gradually, leſs volatile phlogiſton, *i. e.* leſs inflammable air, was expelled; and therefore I ſuppoſe that more of it was fixed. I could never afterwards, by equal degrees of heat, make this coal to weigh as little as another that was firſt coaled by a ſudden heat.

I took two pieces of dry oak, the contiguous parts of the ſame ſtick, each weighing exactly fourteen grains. One of theſe I heated ſuddenly. It yielded eight ounce-meaſures of inflammable air, and then weighed two grains. The other I heated ſlowly, but as vehemently, at the laſt, as the other. It yielded only 1 $\frac{1}{2}$ ounce-meaſures, and weighed three grains.

I repeated the ſame experiment ſeveral times, and always with nearly the ſame reſult.

Examining the conducting power of the pieces of charcoal made with theſe different circumſtances in the proceſs, I could not diſtinguiſh which was better. Perhaps a more accurate method of trying them might ſhow, that thoſe which were coaled ſlowly were the better conductors; unleſs, which is not improbable, the goodneſs of the conducting power conſiſts in the *completeneſs of the union* that is produced between the inflammable principle and its baſe, which will depend upon the *degree of heat* only, and not on the *quantity of phlogiſton* thus united to the earth.

N. B. To catch the inflammable air, ſet looſe in making charcoal, I put the ſubſtances into a gun-barrel, to which I luted a long glaſs tube, and to the tube I faſtened a bladder, out of which the air was carefully preſſed.

As metals and charcoal agree in conſiſting of phlogiſton united to an earthy baſe, and alſo in conducting electricity, I ſuſpected that theſe two different ſubſtances might alſo agree in their readineſs to expand by heat. Mr. Smeaton was ſo obliging as to aſſiſt me in my attempts to aſcertain this circumſtance, by the application of his excellent pyrometer. Though we could not make the experiment with all the exactneſs that we could have wiſhed, yet the reſult of near thirty trials was uniformly in favour of the greater degree of expanſion, by heat, in the charcoal, than in wood of the ſame kind (as we imagined) out of which it was made. In general, the expanſion of the charcoal was about double to that of the wood.

It is evident, that a certain degree of heat makes wood and charcoal expand; and alſo that a greater degree of heat makes them contract. I wiſh we had an inſtrument to aſcertain the preciſe degree of heat, at which the expanſion ceaſes, and the contraction begins; and whether the two effects be produced by the ſame gradation.

In the courſe of theſe experiments on charcoal, I met with a ſubſtance, the conducting power of which is ſingular, and exhibits a beautiful appearance. In order to ſee what would remain after burning a quantity of turpentine in a glaſs tube, I covered it with ſand, in a crucible, in the ſame manner in which I uſed to make charcoal; and, after letting it continue a ſufficient time, in a very hot fire, and after the flame had long ceaſed, I examined the tube, and found that it had been melted; but, inſtead of any thing like charcoal, or the leaſt blackneſs, I obſerved that the tube was uniformly lined with a *whitiſh gloſſy matter*, which I could not ſcrape off. Upon trying whether it would conduct electricity, I found it tranſmitted the ſmalleſt ſhocks, to a conſiderable diſtance; and, what appeared very remarkable, the path of the exploſion was luminous all the way, and ſeemed to conſiſt of a prodigious number of ſmall ſeparate ſparks, ſcattered to a great diſtance, exhibiting ſuch an appearance as would be made by firing gunpowder ſcattered careleſsly in a line. The exploſion very much reſembled the firing of a ſquib. To compare it to another electric appearance, it was like the exploſion paſſing over a thin ſurface of gilding.

I imagine that, though I could not perceive any interruption in this white coating, not even by the help of a microſcope, it muſt, in fact, have been full of interſtices, and the electric ſparks could only be viſible in paſſing from one conducting particle to another.

In this experiment, I often got pieces of glaſs very imperfectly covered, with intervals in the white coating very large and viſible; but, though I expoſed the ſame pieces of glaſs to catch more of this matter, I never could get a coating of it ſo thick, but that, in tranſmitting the electrical exploſion through it, it exhibited the ſame luminous appearance, as if there were interſtices in the circuit.

I got the ſame matter from oil of *turpentine*, and *oil of olives*; but not from *bees-wax*, or *ſpermaceti oil.* Perhaps it cannot be got from any animal ſubſtance.

In order to obſerve the progreſs of this incruſtation, I poured oil of turpentine on ſome flat pieces of glaſs, and burned them on an iron plate, in the open fire, the heat being moderate; but the effect was a black covering, like ſoot, which would not conduct in the leaſt. But theſe ſame pieces of glaſs, thus covered with the black coating, being put into a crucible full of ſand, and urged with a ſtrong heat, came out white, and conducted exactly as before.

With a leſs degree of heat, the black covering was changed to white; but it did not adhere ſo firmly to the glaſs as when the heat had been greater; though it adhered more cloſely than the black covering, which might be wiped off with a feather. But this white coating, produced by a moderate heat, would not conduct at all.

In ſome caſes I have found this whitiſh matter to be diſperſed by ſeveral exploſions, as Dr. Franklin found gilding with leaf-gold to be.

In whatever manner the pieces of glaſs were covered, the coating vaniſhed when it was made red-hot in an open fire; and the glaſs that remained would not conduct, any more than it did before. This circumſtance exactly reſembled the eſcape of phlogiſton from charcoal and metal, burned in the open air.

In a microſcope, this whitiſh matter looked exactly like metal, or rather ſome of the ſemi-metals, having a bright poliſh, though it ſoon became, as it were, tarniſhed.

To try whether it was metal, I dipped the pieces of glaſs that were covered with it in the *acids*, but found that they had little or no effect upon it, though it is by no means fixed in the pores of the glaſs, but covers it quite ſuperficially.

It was not in the leaſt affected by the *magnet*. Upon the whole, the matter that forms this coating of the glaſs ſeems to be a kind of charcoal, only white inſtead of black.

Conſidering that metals reſemble charcoal, in that they conſiſt of an earth united to phlogiſton, and that charcoal will not conſume without burning in the open air (there being, probably, ſomething in the atmoſphere with which it can unite, on the principle of chymical affinities, the moment it is ſeparated from the metallic baſe) I imagined that metals might not calcine, or vitrify, except in the ſame circumſtances, and the event verified my conjecture.

I took a certain quantity of *lead*, and having put it into an open crucible, obſerved that it was all vitrified in ten minutes; but the ſame quantity of lead, covered with pipe-clay, and ſand, was kept ſeveral hours in a much hotter fire, and was hardly waſted at all, the bottom of the crucible only being ſlightly glazed; it having been impoſſible wholly to exclude all acceſs of air, and ſome being neceſſarily in contact with it when the proceſs began. Treating charcoal in the ſame manner, I could never prevent ſome loſs of weight, when the crucible was kept in a very hot fire, for ſeveral hours.

As, by this proceſs, lead will bear a much greater degree of heat than would calcine, or vitrify it, in the open air, I ſhould think it probable, that lead thus prepared muſt have the phlogiſton more cloſely united to its earthy baſe, and be thereby a better conductor than common lead; ſince this is the caſe with charcoal thus treated. Perhaps lead, and other baſe metals, may have their *quality* altered, and be improved in other reſpects alſo by this proceſs; though they ſhould not be changed into gold by it. I found, however, that the ſpecific gravity is not changed by this proceſs; ſo that, alas! it is ſtill but lead.

Fig. 1

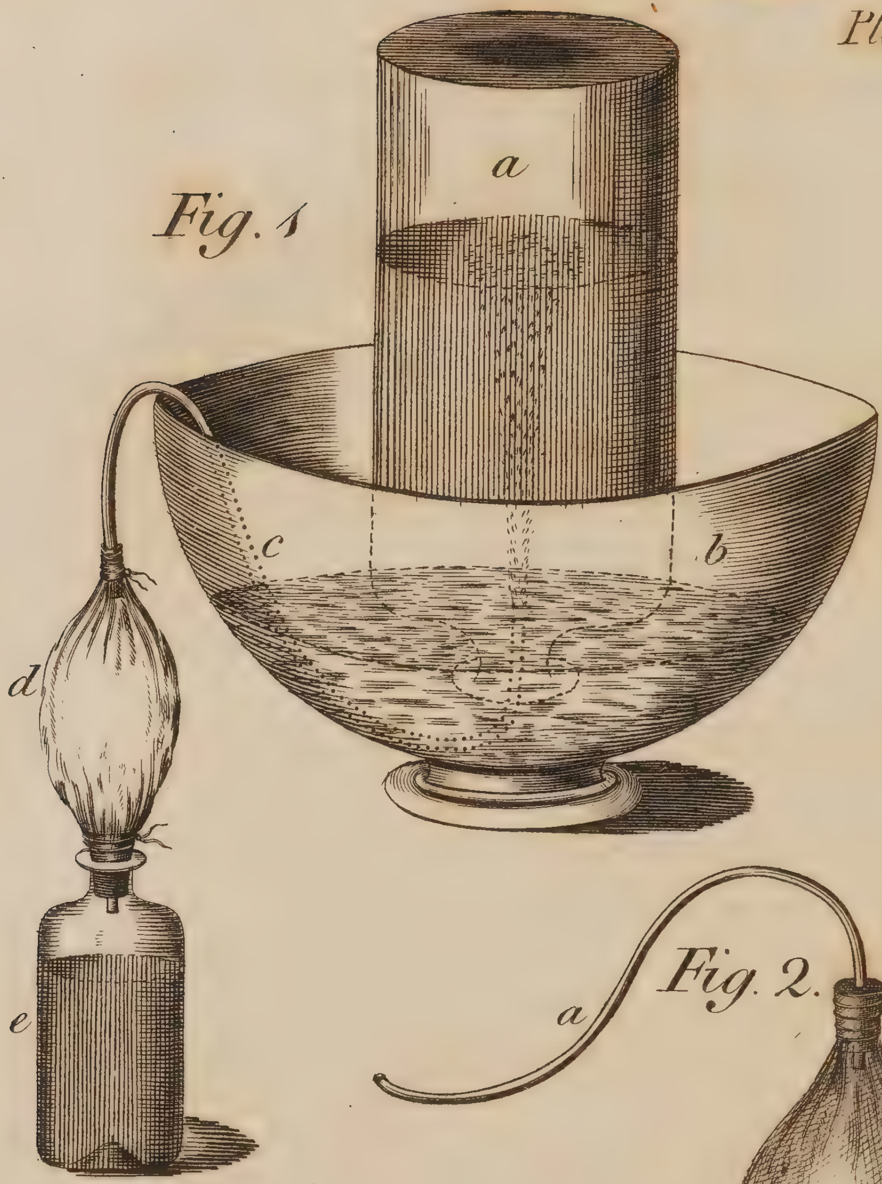


Fig. 2.

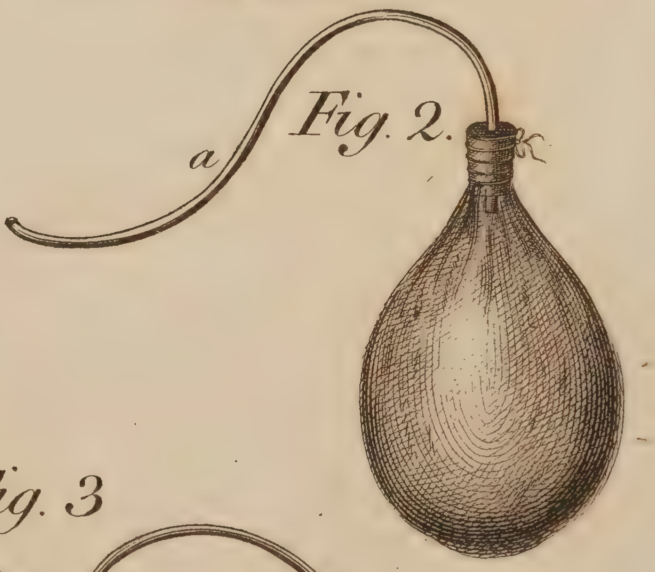
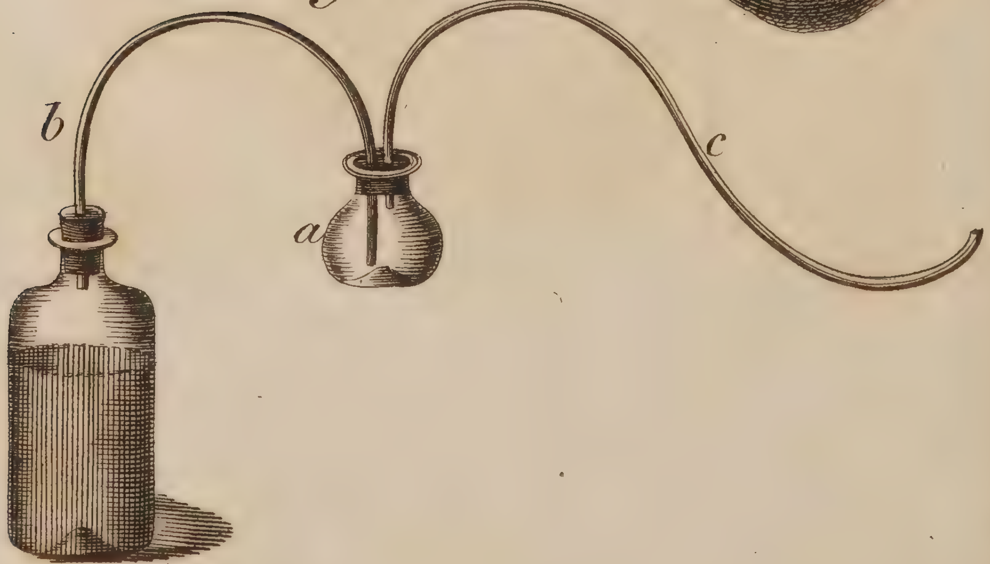


Fig. 3



SECTION XV.

Of the Impregnation of WATER *with* FIXED AIR.

PART I.

The Hiſtory of the Diſcovery.

It often amuſes me when I review the hiſtory of experimental philoſophy, to obſerve how very nearly one diſcovery is connected with another, and yet that, for a long time, no perſon ſhall have perceived that connection, ſo as to have been actually led from the one to the other; and eſpecially that he who made the firſt diſcovery ſhould ſtop ſhort in his progreſs, and not advance a ſingle ſtep farther, to make the other, which was perhaps of infinitely more conſequence. And yet the caſe may be ſuch, that it ſhall be ſo far from requiring more genius, or ingenuity, to advance that other ſtep, that it is rather a matter of wonder, how it was poſſible for the moſt common capacity to ſtop ſhort of it. We alſo frequently find that they who make the moſt important philoſophical diſcoveries overlook the moſt obvious *uſes* of them. Several ſtriking examples of this kind will be found in my *Hiſtory of electricity*, and alſo in the *Hiſtory of diſcoveries relating to viſion, light, and colours.*

In ſuch caſes as theſe it behoves an hiſtorian to be much upon his guard, leſt he ſhould haſtily conclude that to have been fact which he only *imagines* muſt have been ſo, but for which no direct evidence can be produced. As this is a caſe of ſome curioſity reſpecting the human mind, I ſhall give an inſtance of it; and I am able to produce a very remarkable one relating to the ſubject of this ſection.

When it was diſcovered that the acidulous taſte and peculiar virtues of Pyrmont water, and other mineral waters of a ſimilar nature, were owing to the fixed air which they contained; when this air had been actually expelled from the water, and it was found that the ſame water, and even other water, would reimbibe the ſame air; we are apt to conclude, that the perſon who made theſe diſcoveries, and eſpecially the laſt of them (who alſo muſt have known that fixed air is a thing very eaſy to be procured) muſt have immediately gone to work to reduce this *theory* into *practice*, by actually impregnating common water with fixed air, in order to give it the peculiar virtues of thoſe medicinal mineral waters which are ſo highly, and ſo juſtly valued, and which are procured at ſo great an expence, eſpecially in this country. Accordingly, Dr. Nooth has advanced, Phil. Tranſ. vol. 65, p. 59, that "the poſſibility of impregnating water with "fixed air was no ſooner aſcertained by expe-"riment, than various methods were con-"trived to effect the impregnation;" and I doubt not this ingenious philoſopher impoſed upon himſelf in the manner deſcribed above. This, however, is ſo far from being the caſe, that I do not believe it is poſſible to produce the leaſt evidence that any perſon had the thing in view before the publication of my pamphlet upon that ſubject, in the year 1772.

Indeed, had this thing been ſo much as *an object of attention* to philoſophers, it is impoſſible but that ſome of them muſt have hit upon a method that would have ſufficiently ſucceeded. Nay, the thing is ſo very eaſy, and the end attainable in ſo many ways, that there muſt have been, in a very ſhort time, a great variety of methods to impregnate water with fixed air, as there are now; and we ſhould certainly have heard of *artificial mineral waters* being made according to them. It is impoſſible not to conclude ſo, when we conſider the *time that has elapſed* ſince the publication of all the diſcoveries that led to it.

Dr. Brownrigg's paper, giving an account of his diſcovery of fixed air in the Spa water, was read at the Royal Society June the 13th 1765, and was publiſhed in 1766. This excellent philoſopher compleatly decompoſed that mineral water, but he gives no hint of his having ſo much as attempted to *recompoſe* it, or of making a ſimilar water, by impregnating common water with the ſame volatile principle. It is ſufficiently evident that he had not thought of this, though we may wonder that he ſhould not have done it, becauſe he has not mentioned it as an object of purſuit.

In the year following, Mr. Cavendiſh's valuable papers on the ſubject of factitious air were publiſhed. He firſt aſcertained how much fixed air a given quantity of water could be made to imbibe; yet it does not appear that he ever thought of *taſting* the water, much leſs that he thought of making any *practical uſe* of his diſcovery.

If any negative argument can be deciſive, it is that in 1772, the very year in which my pamphlet came out, Dr. Falconer publiſhed his excellent and elaborate treatiſe on the *Bath waters*, in which he treats very largely of mineral waters in general, and all their poſſible impregnations; and yet, though he treats of *fixed air* as one ingredient in many of them, ſee p. 185, he drops no hint about compoſing ſuch water, by imparting fixed air to common water. Alſo on the 12th of September in the ſame year, Dr. Rutherford publiſhed his ingenious *Diſſertation on Fixed Air*, in which he ſpeaks of the preſence of it in Pyrmont water, p. 3, but without giving the leaſt hint of his being acquainted with any method of imitating them. And yet Dr. Nooth ſays, in fact, that from the year 1766, at the lateſt, *various methods* were contrived to effect the impregnation, though he allows that I was the only perſon who "publiſhed any deſcription of an "apparatus calculated entirely for this pur"poſe."

According to this account of the matter there were, in the interval between 1766 and 1772, a ſpace of ſix years, a variety of methods for impregnating water with fixed air, ſome of them prior to, and perhaps much better than mine (though he gives no hint of his own having been invented in that period, but ſpeaks of it as ſuggeſted by the conſideration of the imperfection of mine) but that I happened to get the ſtart in the publication. Dr. Falconer, however, though the friend of Dr. Nooth (ſee his Treatiſe on Bath Water, vol. 2. p. 323) had certainly never heard of any of thoſe methods, or even of mine, at the very termination of that period; and tho' my own acquaintance with philoſophical and medical people is pretty extenſive, I never heard of any of the *various methods* that Dr. Nooth ſpeaks of; nor ſince the publication of my method have I heard of any perſon whatever having pretended to have done the ſame thing before; though nothing is more common than ſuch claims, and very often on the moſt trifling pretences.

Mr. Venelle, indeed, immediately upon the tranſlation of my pamphlet into French, which was within a few weeks after the publication of it in Engliſh (owing to the laudable zeal of Mr. Trudaine, for promoting all philoſophical and uſeful improvements) publiſhed an extract of his papers from the *Memoires de Mathematique & de Phyſique*, to vindicate to himſelf not my diſcovery, but, in fact, that of Dr. Brownrigg. However, what he pretends to have diſcovered was, that the virtues of the acidulous waters were owing to *air*, *in general*, without having any idea of the difference between fixed air and common air; ſo that his diſcovery was ſo far from being the ſame with mine, that it could not poſſibly have led into it.

As I have hitherto only publiſhed the method of impregnating water with fixed air in a ſmall pamphlet, for the uſe of thoſe who might chuſe to reduce it into practice, without giving any account of the manner in which the diſcovery (if it deſerves to be called one) was made, which has been my cuſtom with reſpect to every thing elſe, I ſhall do it here; and I hope the narrative will not be altogether diſpleaſing, as this buſineſs has gained ſo much attention in all parts of Europe, as well as in England, and promiſes in a ſhort time to ſave the very great expence of tranſporting acidulous waters to conſiderable diſtances, by ſuperſeding, in a great meaſure, the uſe of them. And though what I have done in this buſineſs has certainly the leaſt merit poſſible with reſpect to *ingenuity*, I ſhall always conſider it as one of the *happieſt* thoughts that ever occurred to me; becauſe it has proved to be of very ſignal *benefit* to mankind, and will, I doubt not, be of much more conſequence in a courſe of time.

It was a little after Midſummer in 1767, that I removed from Warrington to Leeds; and living, for the firſt year, in a houſe that was contiguous to a large common brewery, ſo good an opportunity produced in me an inclination to make ſome experiments on the fixed air that was conſtantly produced in it. Had it not been for this circumſtance, I ſhould, probably, never have attended to the ſubject of air at all. Happening to have read Dr. Brownrigg's excellent paper on the Spa water about the ſame time, one of the firſt things that I did in this brewery was to place ſhallow veſſels of water within the region of fixed air, on the ſurface of the fermenting veſſels; and having left them all night, I generally found, the next morning, that the water had acquired a very ſenſible and pleaſant impregnation; and it was with peculiar ſatisfaction that I firſt drank of this water, which I believe was the firſt of its kind that had ever been taſted by man.

This proceſs, however, was very ſlow. But after ſome time it occurred to me, that the impregnation might be accelerated, by pouring the water from one veſſel into another, while they were both held within the ſphere of the fixed air; and accordingly I found that I could do as much in about five minutes in this way, as I had been able to do in many hours before. Several of my friends who viſited me while I lived in that houſe will remember my taking them into that brewery, and giving them a glaſs of this artificial Pyrmont water, made in their preſence. Among others, I will take the liberty to mention John Lee, Eſq; of Lincoln's Inn, who was particularly ſtruck with the contrivance, and the effect of it. This was in the ſummer of the year 1768.

One would naturally think, that having actually impregnated common water with fixed air, produced in a brewery, I ſhould immediately have ſet about doing the ſame thing with air ſet looſe from chalk, &c. by ſome of the ſtronger acids; and I do remember that it did occur to me that the thing was poſſible. But, eaſy as the practice proved to be, no method of doing it at that time occurred to me. I ſtill continued to make my Pyrmont water in the manner abovementioned till I left that ſituation, which was about the end of the ſummer 1768; and from that time, being engaged in other ſimilar purſuits, with the reſult of which the public are acquainted, I made no more of the Pyrmont water till the ſpring of the year 1772.

In the mean time I had acquainted all my friends with what I had done, and frequently expreſſed my wiſhes that perſons who had the care of large *diſtilleries* (where I was told that fermentation was much ſtronger than in common breweries) would contrive to have veſſels of water ſuſpended within the fixed air, which they produced, with a farther contrivance for agitating the ſurface of the water; as I did not doubt but that, by this means, they might, with little or no expence, make great quantities of Pyrmont water; by which they might at the ſame time both ſerve the Public, and benefit themſelves. For I never had the moſt diſtant thought of making any advantage of the ſcheme myſelf.

In all this time, viz. from 1767 to 1772, I never heard of any method of impregnating water with fixed air but that abovementioned. My thinking at all of reducing to practice any method of effecting this, by air diſlodged from chalk, and other calcareous ſubſtances, was owing to a mere accident. Being at dinner with the Duke of Northumberland, in the ſpring of the year laſt mentioned, his Grace produced a bottle of water diſtilled by Dr. Irving for the uſe of the navy. This water was perfectly ſweet, but, like all diſtilled water, wanted the briſkneſs and ſpirit of freſh ſpring water; when it immediately occurred to me that I could eaſily mend that water for the uſe of the navy, and perhaps ſupply them with an eaſy and cheap method of preventing or curing the ſea ſcurvy, viz. by impregnating it with fixed air. For having been buſy about a year before with my experiments on air, in the courſe of which I had aſcertained the proportional quantity of ſeveral kinds of air that given quantities of water would take up, I was at no loſs for the *method* of doing it in general, viz. inverting a jar filled with water, and conveying air into it from bladders previouſly filled with air. This ſcheme I immediately mentioned to the Duke and the company, who all ſeemed to be much pleaſed with it, and expreſſed their wiſhes that I would attend to it, and endeavour to reduce it into practice; which I promiſed to do.

The next day I provided a ſmall apparatus, adapted to this purpoſe, at my lodgings, which was very eaſy, as it required no other veſſels but ſuch as are in conſtant family uſe, and with this I preſently impregnated a quantity of the New River water, ſo as to make it imbibe about its bulk of air. But I was far from having hit upon the *eaſieſt method* of doing it; for my jars were of an equal width throughout. However, with theſe veſſels the proceſs was compleated in about twenty minutes, or half an hour.

A few days after this, having an invitation to wait upon Sir George Savile, I carried with me a bottle of my impregnated water, and told him the uſe that might be made of it, viz. that of ſupplying a pleaſant and wholeſome beverage for ſeamen, and ſuch as might probably prevent or cure the ſea-ſcurvy. Sir George, with that warmth with which he eſpouſes every thing that he conceives to be for the public good, inſiſted upon writing a card immediately to Lord Sandwich, propoſing to introduce me to him, as having *a propoſal for the uſe of the navy.* As I could make no objection, the card was accordingly written, and an anſwer was preſently returned from his Lordſhip, informing us that he would be glad to ſee us the next day. Upon this I drew up ſomething in the form of a *propoſal*, which, accompanied by Sir George, I preſented to his Lordſhip, who promiſed to lay it before the Board of Admiralty.

Preſently after this I had notice from the Secretary to the Board of Admiralty, that the *College of Phyſicians* were appointed to examine my propoſal, and to make their report of it to the Board, and an early day was fixed for me to wait upon them at their hall in Warwick-Lane; where, before a very full meeting, I produced a bottle of my impregnated water, and alſo, at their requeſt, fetched my apparatus, and ſhewed them the manner in which I had impregnated it. There were preſent ſeveral of the moſt eminent phyſicians in

London; but both the *ſcheme*, and the *object* of it, appeared to be intirely new to every one of them; and moſt of them ſeemed to be much pleaſed with it.

Accordingly, a favourable report was made to the Board of Admiralty, and I was acquainted by the Secretary, that the captains of the two ſhips which were juſt then ſailing for the South-Seas had orders to make trial of the impregnated water; and for their uſe I drew out my *Directions* in writing, and ſent a drawing of the neceſſary apparatus. The method which I had now got into was a great improvement upon that which I had made uſe of before the College of Phyſicians. For, in conſequence of giving more attention to it, I had, by that time, brought it to the ſtate in which it is deſcribed in the pamphlet.

In the mean time, I had, before I left London, in the ſpring of that year, made the experiment of the impregnation of water with fixed air in the preſence of moſt of my philoſophical acquaintance, and their friends, both at my own lodgings, and in other places. But upon none of theſe occaſions did it appear that any of them had heard of any other perſon having had the ſame thing in view.

Laſtly, I will obſerve, that Sir John Pringle, in his *Diſcourſe on different kinds of air* (in which he has, with the greateſt exactneſs, aſſigned to every perſon concerned in theſe diſcoveries their due ſhare of praiſe) gives no hint of his being acquainted with any other method of impregnating water with fixed air, than that which I had publiſhed. He certainly had not heard of any of thoſe to which Dr. Nooth alludes.

As I have not to this day, directly or indirectly, made the leaſt advantage of this ſcheme; but, on the contrary, am juſt ſo much a loſer by it as the experiments coſt me, I think it is not too much for the Public to allow me, what I believe is ſtrictly my due, *the ſole merit of the diſcovery*; which with reſpect to *ingenuity*, or ſagacity, is next to nothing; but with reſpect to its *utility* is, unqueſtionably, of unſpeakable value to my country and to mankind.

PART II.

DIRECTIONS *for impregnating* WATER *with* FIXED AIR.

SECT. I. *The Preface to the Directions as firſt publiſhed.*

The method of impregnating water with fixed air, of which a deſcription is given in this pamphlet, I hit upon in a courſe of experiments, an account of which was lately communicated to the Royal Society; containing obſervations on ſeveral different kinds of air, with only a hint of the method of combining this particular kind with water or other fluids. Judging that water thus impregnated with fixed air muſt be particularly ſerviceable in long voyages, by preventing or curing the ſea-ſcurvy, according to the theory of Dr. Macbride, and all the Phyſicians of my acquaintance concurring with me in that opinion, I made the firſt communication of it to the Lords of the Admiralty, who referred me to the College of Phyſicians; and thoſe gentlemen being pleaſed to make a report favourable to the ſcheme, a trial has been ordered to be made of it on board ſome of his majeſty's ſhips. To make this proceſs more generally known, and that more frequent trials may be made by water thus medicated, at land as well as at ſea, I have been induced to make the preſent publication.

Sir John Pringle firſt obſerved, that putrefaction was checked by fermentation, and Dr. Macbride diſcovered that this effect was produced by the fixed air which is generated in that proceſs, and upon that principle recommended the uſe of *wort*, as ſupplying a quantity of this fixed air, by fermentation in the ſtomach, in the ſame manner as it is done by freſh vegetables, for which he, therefore, thought that it would be a ſubſtitute; and experience has confirmed his conjecture. Dr. Black found that lime-ſtone, and all calcareous ſubſtances, contain fixed air, that the preſence of it makes them what is called *mild*, and that the deprivation of it renders them *cauſtic*; Dr. Brownrigg farther diſcovered that Pyrmont, and other mineral waters, which have the ſame acidulous taſte, contain a conſiderable proportion of this very kind of air, and that upon this their peculiar ſpirit and virtues depend; and I think myſelf fortunate in having hit upon a very eaſy method of communicating this air to any kind of water, or, indeed,

deed, to almoſt any fluid ſubſtance. In ſhort, by this method this great antiſeptic principle may be adminiſtered in a variety of agreeable vehicles.

If this diſcovery (though it doth not deſerve that name) be of any uſe to my countrymen, and to mankind at large, I ſhall have my reward. For this purpoſe I have made the communication as early as I conveniently could, ſince the lateſt improvements that I have made in the proceſs; and I cannot help expreſſing my wiſhes, that all perſons, who diſcover any thing that promiſes to be generally uſeful, would adopt the ſame method.

SECT. 2. *The Directions.*

If water be only in contact with fixed air, it will begin to imbibe it, but the mixture is greatly accelerated by agitation, which is continually bringing freſh particles of air and water into contact. All that is neceſſary, therefore, to make this proceſs expeditious and effectual, is firſt to procure a ſufficient quantity of this fixed air, and then to contrive a method by which the air and water may be ſtrongly agitated in the ſame veſſel, without any danger of admitting the common air to them; and this is eaſily done by firſt filling any veſſel with water, and introducing the fixed air to it, while it ſtands inverted in another veſſel of water. That every part of the proceſs may be as intelligible as poſſible, even to thoſe who have no previous knowledge of the ſubject, I ſhall deſcribe it very minutely, ſubjoining ſeveral remarks and obſervations relating to varieties in the proceſs, and other things of a miſcellaneous nature.

The Preparation.

Take a glaſs veſſel, *a*, pl. 2. fig. 1. with a pretty narrow neck, but ſo formed, that it will ſtand upright with its mouth downwards, and having filled it with water, lay a ſlip of clean paper, or thin paſteboard, upon it. Then, if they be preſſed cloſe together, the veſſel may be turned upſide down, without danger of admitting common air into it; and when it is thus inverted, it muſt be placed in another veſſel, in the form of a bowl or baſon, *b*, with a little water in it, ſo much as to permit the ſlip of paper or paſteboard to be withdrawn, and the end of the pipe *c* to be introduced.

This pipe muſt be flexible, and air-tight, for which purpoſe it is, I believe, beſt made of leather, ſewed with a waxed thread, in the manner uſed by ſhoe-makers. Into each end of this pipe a piece of a quill ſhould be thruſt, to keep them open, while one of them is introduced into the veſſel of water, and the other into the bladder *d*, the oppoſite end of which is tied round a cork, which muſt be perforated, the whole being kept open by a quill; and the cork muſt fit a phial *e*, two thirds of which ſhould be filled with chalk juſt covered with water.

I have ſince, however, found it moſt convenient to uſe a *glaſs tube*, and to preſerve the advantage which I had, of agitating the veſſel *e*, I have *two bladders*, communicating by a perforated cork, to which they are both tied. For one bladder would hardly give room enough for that purpoſe.

The Proceſs.

Things being thus prepared, and the phial containing the chalk and water being detached from the bladder, and the pipe alſo from the veſſel of water, pour a little oil of vitriol upon the chalk and water; and having carefully preſſed all the common air out of the bladder, put the cork into the bottle preſently after the effervеſcence has begun. Alſo preſs the bladder once more after a little of the newly generated air has got into it, in order the more effectually to clear it of all the remains of the common air; and then introduce the end of the pipe into the mouth of the veſſel of water as in the drawing, and begin to agitate the chalk and water briſkly. This will preſently produce a conſiderable quantity of fixed air, which will diſtend the bladder; and this being preſſed, the air will force its way through the pipe, and aſcend into the veſſel of water, the water at the ſame time deſcending, and coming into the baſon.

When about one half of the water is forced out, let the operator lay his hand upon the uppermoſt part of the veſſel, and ſhake it as briſkly as he can, not to throw the water out of the baſon; and in a few minutes the water will abſorb the air; and taking its place, will nearly fill the veſſel as at the firſt. Then ſhake the phial containing the chalk and water again, and force more air into the veſſel, till, upon the whole, about an equal bulk of air has been thrown into it. Alſo ſhake the water as before, till no more of the air can be imbibed. As ſoon as this is perceived to be the caſe, the water is ready for uſe; and if it be not uſed immediately, ſhould be put into a bottle as ſoon as poſſible, well corked, and cemented. It will keep, however, very well, if the bottle be only well corked, and kept with the mouth downwards.

Obſervations.

1. The baſon may be placed inverted upon the veſſel full of water, with a ſlip of paper between them, and then both turned upſide down together; but all this trouble will be ſaved by having a larger veſſel of water, in which both of them may be immerſed.

2. If the veſſel containing the water to be agitated be large, it may be moſt convenient firſt to place it inverted, in a baſon full of water, and then to draw out the common air by means of a ſyphon, either making uſe of a ſyringe, or drawing it out with the mouth. In this caſe, alſo, ſome kind of handle ſhould be faſtened to the bottom of the veſſel, for the more eaſy agitation of it.

3. A narrow mouthed veſſel is not neceſſary, but it is the moſt proper for the purpoſe, becauſe it may be agitated with leſs danger of the common air getting into it.

4. The flexible pipe is not neceſſary, though I think it is exceedingly convenient. When it is not uſed, a bent tube, *a*, fig. 2. (for which glaſs is the moſt proper) muſt be ready to be inſerted into the hole made in the cork, when the bladder containing the fixed air is ſeparated from the phial, in which it was generated. The extremity of this tube being put under the veſſel of water, and the bladder being compreſſed, the air will be conveyed into it, as before.

5. If the uſe of a bladder be objected to, though nothing can be more inoffenſive, the phial containing the chalk and water muſt not be agitated at all, or with the greateſt caution; unleſs a ſmall phial, *a*, fig. 3. be interpoſed between the phial and the veſſel of water, in the manner repreſented in the drawing. For by this means the chalk and water that may be thrown up the tube *b* will lodge at the bottom of the phial *a*, while nothing but the air will get into the pipe *c*, and ſo enter the water. If the tube *b* be made of tin or copper, the ſmall phial *a* will not need any other ſupport, the cork into which the extremities of both the tubes are inſerted being made to fit the phial very exactly.

6. The phial *e*, fig. 1. ſhould always be placed, or held, conſiderably lower than the veſſel *a*; that if any part of the mixture ſhould be thrown up into the bladder, it may remain in the lower part of it, from which it may be eaſily preſſed back again. This, however, is not neceſſary, ſince if it remain in the lower part of the bladder, nothing but the pure air will get into the pipe, and ſo into the water.

7. If much more than half of the veſſel be filled with air, there will not be a body of water ſufficient to agitate, and the proceſs will take up much more time.

8. If the chalk be too finely powdered, it will yield the fixed air too faſt.

9. After every proceſs, the water to which the chalk is put muſt be changed.

10. It will be proper to fill the bladder with water once every day, after it has been uſed, that any of the oil of vitriol which may have got into it, and would be in danger of corroding it, may be thoroughly diluted.

11. The veſſel, which I have generally made uſe of, holds about three pints, and the phial containing the chalk and water is one of ten

ounces; and I find that a little more than a tea-ſpoonful of oil of vitriol is ſufficient to produce as much air as will impregnate that quantity of water.

12. If the veſſel containing the water be larger, the phial containing the chalk and the oil of vitriol ſhould either be larger in proportion, or freſh water and oil of vitriol muſt be put to the chalk, to produce the requiſite quantity of air.

13. In general, the whole proceſs does not take up more than about a quarter of an hour, the agitation not five minutes; and in nearly the ſame time might a veſſel of water, containing two or three gallons, or indeed any quantity that a perſon could well ſhake, be impregnated with fixed air, if the phial containing the chalk and oil of vitriol, be larger in the ſame proportion.

14. To give the water as much air as it can receive in this way, the proceſs may be repeated with the water thus impregnated. I generally chuſe to do it two or three times, but very little will be gained by repeating it oftener; ſince, after ſome time, as much fixed air will eſcape from that part of the ſurface of the

water which is expoſed to the common air, as can be imbibed from within the veſſel.

15. All calcareous ſubſtances contain fixed air, and any acids may be uſed in order to ſet it looſe from them; but chalk and oil of vitriol are, both of them, the cheapeſt, and, upon the whole, the beſt for the purpoſe.

16. It may poſſibly be imagined that part of the oil of vitriol is rendered volatile in this proceſs, and ſo becomes mixed with the water; but it does not appear, by the moſt rigid chymical examination, that the leaſt perceivable quantity of the acid gets into the water in this way; and if ſo ſmall a quantity as a ſingle drop of oil of vitriol be mixed with a pint of water (and a much greater quantity would be far from making it leſs wholeſome) it might be diſcovered. The experiments which were made to aſcertain this fact were made with *diſtilled water*, the diſagreeable taſte of which is not taken off, in any degree, by the mixture of fixed air. Otherwiſe, diſtilled water, being clogged with no foreign principle, will imbibe fixed air faſter, and retain a greater quantity of it than other water. In the experiments that were made for this purpoſe, I was aſſiſted by Mr. Hey, a ſurgeon in Leeds, who is well

ſkilled in the methods of examining the properties of mineral waters.

17. Doctor Brownrigg, who made his experiments on Pyrmont water at the ſpring head, never found that it contained ſo much as one half of an equal bulk of air; but in this method the water is eaſily made to imbibe an equal bulk. For it muſt be obſerved, that a conſiderable quantity of the moſt ſoluble part of the air is incorporated with the water, as it firſt aſcends through it, before it occupies its place in the upper part of the veſſel.

18. The heat of boiling water will expel all the fixed air, if a phial containing this impregnated water be held in it; but it will often require above half an hour to effect it compleatly.

19. If any perſon would chuſe to make this medicated water more nearly to reſemble genuine Pyrmont water, Sir John Pringle informs me, that from eight to ten drops of *Tinctura Martis cum ſpiritu ſalis* muſt be mixed with every pint of it. It is agreed, however, on all hands, that the peculiar virtues of Pyrmont, or any other mineral water which has the ſame briſk or acidulous taſte, depend not

upon its being a chalybeate, but upon the fixed air which it contains.

But water impregnated with fixed air does of itſelf diſſolve iron, as the ingenious Mr. Lane has diſcovered; and iron filings put to this medicated water make a ſtrong and agreeable chalybeate, ſimilar to ſome other natural chalybeates, which hold the iron in ſolution by means of fixed air only, and not by means of any acid; and theſe chalybeates, I am informed, are generally the moſt agreeable to the ſtomach.

20. By this proceſs may fixed air be given to wine, beer, and almoſt any liquor whatever: and when beer is become flat or dead, it will be revived by this means; but the delicate agreeable flavour, or acidulous taſte communicated by the fixed air, and which is manifeſt in water, will hardly be perceived in wine, or other liquors which have much taſte of their own.

21. I would not interfere with the province of the phyſician, but I cannot intirely ſatisfy myſelf without taking this opportunity to ſuggeſt ſuch hints as have occurred to myſelf, or my friends, with reſpect to the *medicinal uſes*

of water impregnated with fixed air, and alſo of fixed air in other applications.

In general, the diſeaſes in which water impregnated with fixed air will moſt probably be ſerviceable, are thoſe of a *putrid* nature, of which kind is the *ſea-ſcurvy*. It can hardly be doubted, alſo, but that this water muſt have all the medicinal virtues of Pyrmont water, and of other mineral waters ſimilar to it, whatever they be; eſpecially if a few iron filings be put to it, to render it a chalybeate, like genuine Pyrmont water. It is poſſible, however, that, in ſome caſes, it may be deſirable to have the *fixed air* of Pyrmont water, without the *iron* which it contains.

Having this opportunity, I ſhall alſo hint the application of fixed air in the form of *clyſters*, which occurred to me while I was attending to this ſubject, as what promiſes to be uſeful to correct putrefaction in the inteſtinal canal, and other parts of the ſyſtem to which it may, by this channel, be conveyed. It has been tried once by Mr. Hey above-mentioned, and the recovery of the patient from an alarming putrid fever, when the ſtools were become black, hot, and very fetid, was ſo circumſtanced, that it is not improbable but that it might be owing, in ſome meaſure, to thoſe clyſters. The application, however, appeared to be perfectly eaſy and ſafe.

I cannot help thinking that fixed air might be applied externally to good advantage in other caſes of a putrid nature, even when the whole ſyſtem was affected. There would be no difficulty in placing the body ſo, that the greateſt part of its ſurface ſhould be expoſed to this kind of air; and if a piece of putrid fleſh will become firm and ſweet in that ſituation, as Dr. Macbride found, ſome advantage, I ſhould think, might be expected from the ſame antiſeptic application, aſſiſted by the *vis vitæ*, operating internally, to counteract the ſame putrid tendency. Some Indians, I have been informed, bury their patients, labouring under putrid diſeaſes, up to the chin in freſh mould, which is alſo known to take off the fœtor from fleſh meat beginning to putrify. If this practice be of any uſe, may it not be owing to the fixed air imbibed by the pores of the ſkin in that ſituation? Following the plough is alſo an old preſcription for a conſumption, as alſo is living near lime kilns. There is often ſome good reaſon for very old and long continued practices, though it is frequently a long time before it be diſcovered, and the *rationale* of them ſatisfactorily explained.

Being no phyſician, I run no riſque by throwing out theſe random hints and conjectures. I ſhall think myſelf happy, if any of them ſhould be the means of making thoſe perſons, whom they immediately concern, attend more particularly to the ſubject. My friend Dr. Percival has for ſome time paſt been employed in making experiments on fixed air, and he is particularly attentive to the medicinal uſes of it; and from his knowledge as a philoſopher, and ſkill in his profeſſion, I have very conſiderable expectations.

PART III.

Of Dr. NOOTH's *Objections to the preceding Method of impregnating Water with fixed Air, and a Compariſon of it with his own Method, both as publiſhed by himſelf, and as improved by Mr.* PARKER.

I can eaſily forgive Dr. Nooth for his repreſenting me as having no other merit than the *firſt publication* of the method for impregnating water with fixed air, accounting for it as I have done before; but I cannot ſo eaſily forgive another paragraph in his paper, the tendency of which is intirely to diſcredit a method, which, though it is, in ſome reſpects, inferior to his own, has nevertheleſs its peculiar advantages: and every advantage cannot poſſibly concur in any one method. He ſays, p. 59, " Independent of the inconveniencies " attending the proceſs, there was another ob" jection to the apparatus, which, with moſt " people, might have conſiderable weight. " The *bladder*, which formed part of it, was " thought to render the water offenſive; and " when the ſolvent power of fixed air is con" ſidered, it will not appear improbable, that

“ the water would be always more or leſs
“ tainted by the bladder. In ſome trials which
“ I made with Dr. Prieſtley's apparatus, it
“ always happened that the water acquired an
“ *urinous flavour*; and this taſte was, in general, ſo predominant, that it could not be
“ ſwallowed without ſome degree of reluctance.”

That Dr. Nooth *did* produce an impregnated water which he could not ſwallow without reluctance, and even that, in the trials to which he refers, he *generally* produced ſuch water, I am far from doubting; becauſe that might happen from various cauſes. But that the urinous flavour came from the *bladder*, as ſuch, I will venture to ſay is not poſſible. For then it would *always* have had the ſame effect; and not only myſelf have never perceived ſuch a flavour as the Doctor complains of, but this is the only complaint of the kind that I have hitherto heard of; though many perſons of the moſt delicate taſte, and particularly many ladies, have uſed the water impregnated in my method for months together. Few perſons have had to do with bladders, and fixed air confined in bladders, more than myſelf; and yet I have never ſeen any reaſon to ſuſpect this great *ſolvent power* of fixed air with reſpect to them; eſpecially ſo as to be apparent in the ſpace of a few minutes.

But ſuppoſing the fixed air to be capable of diſſolving the whole bladder, and to carry it along with itſelf into the impregnated water, no phyſician, or philoſopher, will pretend to ſay that it could have any more tendency to give it an *urinous flavour*, than if it had been any other membrane of the animal body.

Indeed, as the Doctor himſelf does not pretend to ſay that this ſtrange urinous flavour was the effect of *all* the impregnations of water made in my method, but only in *ſome* of them (though it was *generally* ſo, in thoſe particular trials) it is evident, from his tacit confeſſion, that it muſt have been an *accidental thing*, and could not have come from the bladder, which I ſuppoſe he made uſe of in all trials. For he has not done me the juſtice to acknowledge that, in my pamphlet, among the various methods of effecting the impregnation of water, I have deſcribed one in which no bladder is made uſe of. When the Doctor ſhall once more produce this urinous flavour (and as a new and curious experiment, it is certainly worthy of his farther inveſtigation) taking care that no careleſs ſervant ſhall have mixed any urine in the water that he calls for, I ſhall give this new objection to my proceſs a farther examination. At preſent I am inclined to conſider this as an experiment of the ſervant, rather than of the Doctor himſelf.

Several perſons have thought that fixed air diſcharged from *impure chalk* gives the water that is impregnated with it a diſagreeable flavour, but this I have never obſerved myſelf; and any other calcareous matter may be uſed in my method, as well as in that of Dr. Nooth, who recommends chalk, as the beſt upon the whole.

I ſhall conclude theſe animadverſions with doing what Dr. Nooth ought to have done before me, viz. fairly ſtating the advantages and diſadvantages of our two methods. His method requires *leſs ſkill* in the operator, and *a leſs conſtant attention.* It is alſo *more elegant* and cleanly, I mean with reſpect to the *operator*; for this does not at all affect the *impregnated water.* On theſe accounts I generally recommend and make uſe of his method myſelf, eſpecially as the glaſſes are made with improvements by Mr. Parker. But if Dr. Nooth be candid, he muſt acknowledge that my method requires much *leſs time*, and is much *leſs expenſive*; and therefore muſt be more proper

per when a great quantity of impregnated water is wanted; and eſpecially when there is but little room to make it in.

My method indeed requires a conſtant attendance, but I queſtion whether, upon the whole, more than is neceſſary to be given to Dr. Nooth's method at intervals, if the water be at all agitated; conſidering that mine does not require one-tenth part of the time. And though my method requires ſome little ſkill and addreſs, it is not ſo much, but that many perſons, altogether unuſed to experiments, have, to my knowledge, ſucceeded in it very well, and have made the impregnated water in a conſtant way for their family uſe, and without any aſſiſtance beſides what they got from the printed directions. My apparatus coſts little or nothing, becauſe no veſſels are made for the purpoſe; and both the chalk and the acids are made to go as far as poſſible, by means of the convenient agitation of the veſſel in which they are contained. Whereas Dr. Nooth's method requires a peculiar and expenſive apparatus, and more waſte is unavoidable in the uſe of it. However, for the reaſons abovementioned, I have never recommended my own method for the uſe of a family ſince I have been acquainted with his.

What I have ſaid above is rather applicable to the apparatus as it is made by Mr. Parker, than to that which Dr. Nooth has deſcribed. For Mr. Parker's glaſſes are, in my opinion, conſiderably improved from thoſe of Dr. Nooth. It may be ſaid that the improvements conſiſt in *little things*; but little things may have great effects; and, after the diſcovery of the *firſt method* of accompliſhing this end, all *ſubſequent methods* may be called little things; and they may be endleſsly diverſified, without any great claim of merit. I have ſeen ſeveral very ingenious methods ſince the publication of mine, though none that I like ſo much, upon the whole, as that of Dr. Nooth, improved by Mr. Parker.

In Dr. Nooth's apparatus, if any more air than is wanted be produced, the water will run out of the uppermoſt veſſel. To uſe his own words, p. 63, "Should more air be extricated than is ſufficient, in the conduct of "the proceſs, to fill that veſſel, the water will "run over the top of it, and will continue to "run as long as any air aſcends in the middle "veſſel, or till the ſurface of the water is below the extremity of the bent tube; and in "this caſe the whole would be wet and diſagreeable." But this diſagreeable conſequence can never happen in the uſe of Mr. Parker's glaſſes, becauſe the bent tube in which the uppermoſt veſſel terminates is made of ſuch a length, that the water expelled from the middle veſſel can do no more than nearly fill the uppermoſt, and can never run over; ſo that whereas Dr. Nooth's apparatus requires a conſtant attendance, Mr. Parker's requires none. The materials being once put into it, the proceſs will go on of itſelf, without any farther care; unleſs the operator ſhould chuſe to accelerate the impregnation by now and then letting out the air that is not eaſily abſorbed, and by agitating the water. This I think to be a conſiderable advantage gained by a very eaſy contrivance of Mr. Parker's, overlooked by Dr. Nooth.

Mr. Parker derives another conſiderable advantage from a *channel* which he cuts in the ſtopper of his uppermoſt veſſel, or from a ſtopper with a hole through the middle, which Dr. Nooth has not in his; ſo that either the operator muſt be careful to take it out during the efferveſcence, or it will be driven out, or ſome of the veſſels will burſt, to the great danger of the by-ſtanders; which actually happened in one made by Mr. Parker, before he thought of this method to prevent it. Whereas, through the channel in Mr. Parker's apparatus; the common air eaſily eſcapes from the uppermoſt veſſel, to make room for the water to aſcend; and when, in the continuance of the proceſs, the fixed air riſes through the bent tube into the uppermoſt veſſel, it lodges upon the ſurface of the water in it; and the communication between it and the common air being ſo much obſtructed, they are ſufficiently ſeparated; ſo that even the water in the uppermoſt veſſel has (if the production of air be copious) almoſt as much advantage for receiving the impregnation, as that in the middle veſſel. This advantage Dr. Nooth loſes.

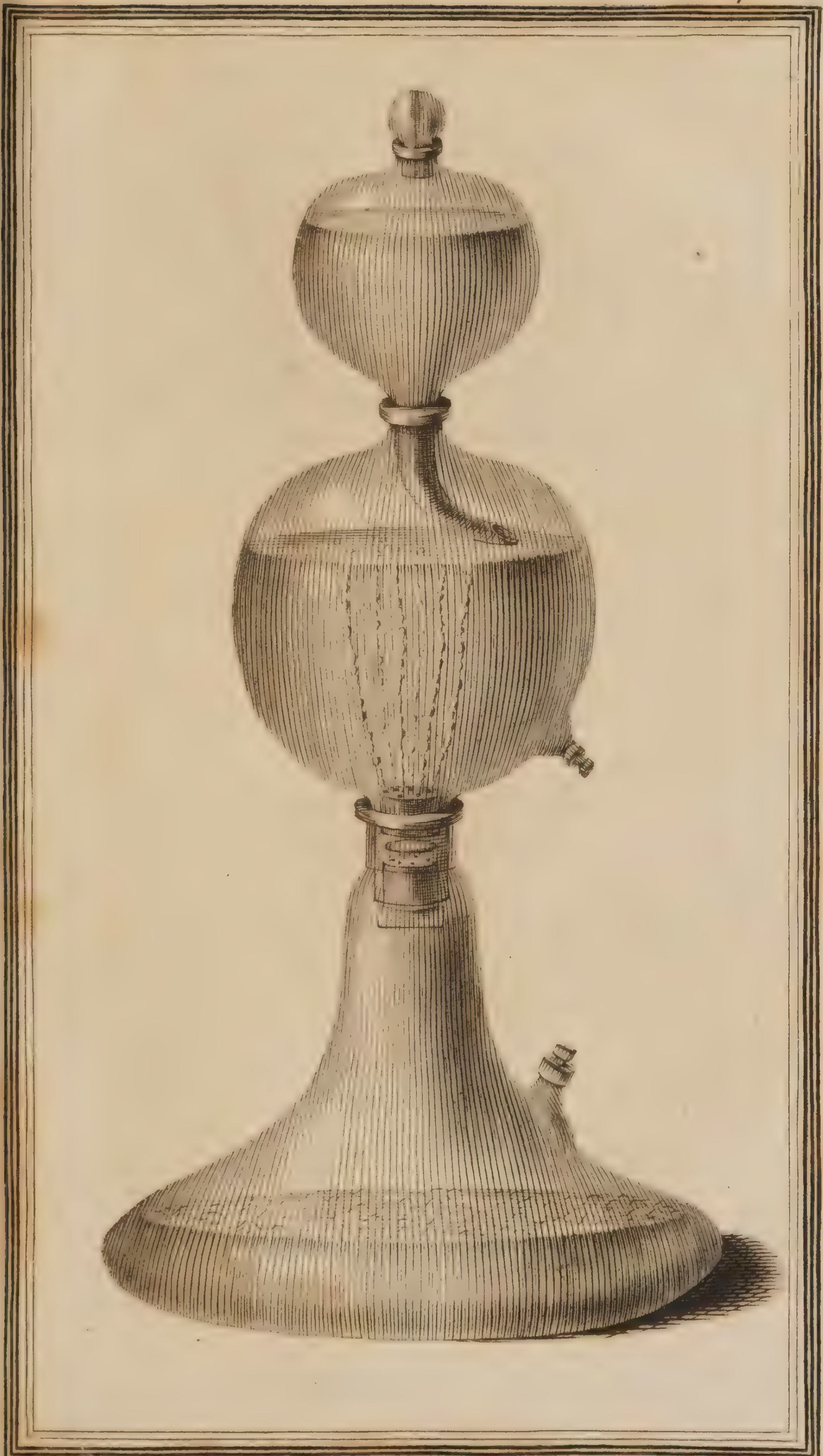
Alſo, when he chuſes to ſeparate the two uppermoſt veſſels from the loweſt, in order to agitate the water, he muſt either leave the mouth of the uppermoſt veſſel open, in which caſe he can hardly agitate the water at all; or (as he prefers to do it) he muſt put the ſtopper in, and conſequently admit the common air to paſs his valve, and mix with the fixed air, which muſt greatly retard the abſorption of it: whereas Mr. Parker's veſſels may be agitated with the ſtopper in, which, admitting the common air into the upper veſſel, through the channel cut in it (or through the hole of the ſtopper) permits the water to deſcend into the lower, on the ſurface of which nothing but fixed air is incumbent. Should any common air enter by the valve, which in this caſe it hardly would, the finger of the perſon who ſhakes the veſſel may eaſily be placed ſo as to prevent it.

Laſtly, I conſider it as a valuable improvement in Mr. Parker's apparatus, that, by means of the openings into the middle and loweſt veſſels, cloſed with ground ſtopples, the operator is enabled to draw off his water, in order to taſte it occaſionally, or to add to his oil of vitriol or chalk, &c. at pleaſure, without giving himſelf the trouble of ſeparating the veſſels from one another for thoſe purpoſes.

The firſt apparatus that I ſaw of Mr. Parker's had no *valve* at all, but only a glaſs ſtopple, with one or more ſmall perforations, for the aſcent of the air into the middle veſſel. This I ſtill generally make uſe of, without finding any occaſion for a valve; the aſcent of the fixed air ſufficiently preventing the deſcent of the water, as long as the proceſs continues, eſpecially when pounded *marble* is uſed. This ſubſtance Dr. Franklin recommended to me, and I give it the preference very greatly to chalk, chiefly on account of the length of time that is required to expel the air from it: For without any freſh acid, it will often continue to yield air for ſeveral days together.

That thoſe perſons who are not poſſeſſed of the Engliſh *Philoſophical Tranſactions*, and particularly foreigners, may underſtand what has preceded, I ſhall in the third plate, in this volume, give a drawing of Dr. Nooth's apparatus, as improved by Mr. Parker, with the following general deſcription of it.

In the loweſt veſſel, the chalk or marble, and the water acidulated with oil of vitriol, muſt be put, and into the middle veſſel the water to be impregnated. During the effervesſcence, the fixed air riſes into the middle veſſel, and reſts upon the ſurface of the water in it, while the water that is diſplaced by the air riſes through the bent tube into the uppermoſt veſſel, the common air going out through the channel in the ſtopple. When the bent tube is of a proper length, the proceſs requires no attention; and if the production of air be copious, the water will generally be ſufficiently impregnated in five or ſix hours. At leaſt, all the attention that needs be given to it is to raiſe the uppermoſt veſſel once or twice, to let out that part of the fixed air which



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which is not readily abſorbed by water. If the operator chuſe to accelerate the proceſs, by agitating the water, he muſt ſeparate the two uppermoſt veſſels from the loweſt. For if he ſhould agitate them all together, he will occaſion too copious a production of air; and he will alſo be in danger of throwing the liquor contained in the loweſt veſſel into contact with the ſtopple which ſeparates it from the middle veſſel, by which means ſome of the oil of vitriol might get into the water.

SECTION XVI.

An Account of ſome Miſrepreſentations of the Author's Sentiments, and of ſome Differences of Opinion with reſpect to the Subject of Air.

I have always flattered myſelf, and the opinion of others has concurred to confirm me in the perſuaſion, that my writings were very intelligible, ſo that few perſons could well miſtake my meaning; and indeed I have no reaſon to complain of my *countrymen* in this reſpect. But I have been ſingularly unfortunate with reſpect to *foreigners*; owing, I ſuppoſe, to their not underſtanding the Engliſh language. For it cannot be that philoſophers, and thoſe whom I conſider as my fellow-labourers in theſe reſearches, ſhould have given ſo little attention to this buſineſs, as to have miſrepreſented my meaning ſo groſsly as they have done, either through a haſty peruſal of my writings, or ſuch an ignorance of the ſubject, as rendered them incapable of underſtanding me; much leſs can it be ſuppoſed that any of them would wilfully miſrepreſent my meaning.

Such, however, is the fact, that, I believe no example can be produced of any perſon whoſe meaning has been ſo egregiouſly miſtaken as mine has been, and even by philoſophers, and writers of great reputation, whoſe works will neceſſarily go into many hands, and conſequently give a very unjuſt and very unfavourable idea of my ſentiments. I think proper, therefore, in this ſection, to enumerate, in as brief a manner as I poſſibly can, not *all* the miſtakes that have been made by all thoſe who have undertaken to give an account of my experiments, for then I muſt have made a book upon the ſubject, but thoſe of a few writers of reputation.

That I do not exaggerate in what I have ſaid above, will not be thought incredible by my reader, when I inform him that in Mr. Roſier's tranſlation of my firſt papers, communicated to the Royal Society, which made no more than a quarto pamphlet, the French tranſlator of the former volume of this work told me (for I have never had patience to read it myſelf) that he had noted fourſcore faults which affected the ſenſe, excluſive of inaccuracies of ſtyle. Theſe fourſcore faults I ſhall therefore intirely paſs over; having, I hope, ſaid enough to caution my reader not to look into that work for an account of any thing that I have ſaid or done. I ſhall not even think it worth while to note all the miſtakes of Mr. Lavoiſier, and I ſhall be as conciſe as poſſible in my remarks, exhibiting what I have been repreſented as ſaying in one column, and what I have really ſaid in another. Mr. Lavoiſier's work is intitled, *Opuſcules Phyſiques & Chymiques*, and will ſoon be publiſhed in Engliſh; Sig. Landriani's is called *Ricerche Fiſiche intorno alla ſalubrita dall'aria.*

Mr. Lavoiſier's Account of my Experiments and Obſervations.	*The true Account of them.*
Monſ. Prieſtley, p. 111, aſſerts a fact which would prove that fixed air is not heavier than common air; ſaying that a candle will continue to burn in a veſſel plunged into an atmoſphere of fixed air with its mouth *upwards*.	On the contrary, I ſaid, p. 27, that the veſſel had its mouth *downwards*; from which a contrary concluſion will follow.
P. 112, he ſays that gunpowder has this *peculiar property*, that being fired in fixed air, the whole of it will incorporate with that air, and that no part of it will eſcape into the common air.	I have only ſaid that this would be the caſe when the quantity of gunpowder was very ſmall, and the body of fixed air on the ſurface of the fermenting liquor very ſtrong.
P. 114, Monſ. Prieſtley aſſerts as an extraordinary fact, that fixed air diminiſhed by iron filings and brimſtone is not noxious to animals, and that it does not differ from common air.	What I have ſaid, p. 42, is that, *in one caſe* only, a quantity of this air was not very noxious to animals; and I attribute that degree of wholeſomeneſs in this air to my having inadvertently agitated it in water, at a time when I was not aware of the effect of

	ſuch agitation; and in the ſame place I obſerve, that in another quantity of fixed air, which had undergone the ſame proceſs, a mouſe died pretty ſoon.
P. 115, One of his experiments would ſeem to prove that there is an acid in fixed air, whereas others of his experiments contradict that opinion.	The *other experiments* are thoſe of Mr. Hey, which were not intended to prove that fixed air is not an acid; but that water impregnated with it is not made acid by any of the oil of vitriol being rendered volatile, and mixing with it.
P. 116, Snails die immediately and irrecoverably in fixed air.	I only mentioned a *ſingle experiment*, p. 36, with one particular ſnail.
P. 119, He transferred very hot air into a receiver, and having placed a candle in it, found that it burned as well as in cold air.	In reality I only put the candle (ſee p. 49) into air that *had been made very hot*, but which was then quite cold.
P. 122, Inflammable air long agitated in water appears to differ in nothing from common air.	I ſaid, p. 68, that a candle burned in this air as in common air, only more faintly; but that, by the teſt of nitrous air, it did not appear to be near ſo good as common air; and that by longer agitation it extinguiſhed a candle.
Ib. Inflammable air from oak has this peculiar quality, that water can abſorb one half of it.	I only ſaid, p. 69, that I made a particular experiment with a quantity of this air, after having agitated it in water till it was diminiſhed about one half. How much more it might have been di-

	miniſhed, by longer agitation, I did not ſay.
P. 123. Inflammable air does not injure the growth of plants.	I only ſaid, p. 61, that plants grew pretty well in one quantity of inflammable air that was made from zinc, and in another from oak.
P. 125, Air injured by reſpiration approaches to the ſtate of fixed air, becauſe it can combine with lime; but it differs from it, becauſe when mixed with common air it diminiſhes the bulk of it: whereas fixed air increaſes it. It is alſo not abſorbed by water, like fixed air.	Air injured by reſpiration will not unite with lime in lime water; though when air is thus injured ſomething is precipitated from it, which has that effect. I have no where ſaid that air injured by reſpiration diminiſhes the quantity of common air with which it is mixed, though that *principle* which had diminiſhed this air will diminiſh any other wholeſome air. Mr. Lavoiſier himſelf quotes me, p. 129, as ſaying, that when I had mixed air injured by putrefaction with common air, the quantity was *not* diminiſhed. If this injured air is not capable of being abſorbed by water, which is the caſe, it muſt differ very eſſentially from fixed air.
P. 130, He has made a great number of experiments, which ſhew that plants vegetating in corrupted air make it as proper for reſpiration as the air of the atmoſphere.	I never pretended to have reſtored air that was *thoroughly noxious* by any method, ſo far as that a candle would burn in it. Though the growth of plants in air which had never been more injured than that a candle would go out in it, (which is a much greater injury than the general maſs of the atmoſphere ever ſuffers)

	never failed to reſtore it ſo far as that a candle would burn in it, to all appearance, as well as ever.
P. 131, Monſ. Prieſtley ſays, that all kinds of noxious air are reſtored by long agitation in water; which ſeems to contradict what he ſays in another place, that agitation in water was not ſufficient to deprive noxious air of its deadly quality.	I ſaid, in an account of an experiment in which I *frequently transferred* a quantity of noxious air from one veſſel to another, that I did not find it to be reſtored by that means. But this is a very different operation from the *agitation* of air in water, and eſpecially when it is continued a long time.
P. 136, Monſ. Prieſtley ſays, that nitrous air may be abſorbed by water; but with reſpect to the quantity of abſorption, the reſults of his experiments do not ſeem to agree exactly.	I have no where ſtated the *ultimum* of the abſorption of nitrous air by water. Indeed, ſtrictly ſpeaking, water deprived of all air will abſorb the whole of any kind of air. I only mentioned different degrees of abſorption, as I obſerved them in circumſtances that were conſiderably different.
P. 137, A paſte of iron filings and brimſtone diminiſhes common air one-fourth: but by means of nitrous air this diminution may be carried much farther. For if a quantity of nitrous air be admitted to this mixture, the common air will be reduced to one-fourth of its bulk.	What I have ſaid is, that the *nitrous air* itſelf, and not *common air*, was diminiſhed to one-fourth of its bulk by the fermentation of that paſte. No common air was concerned in the experiment.
P. 140, By throwing the focus of a burning mirror on a piece of charcoal in com-	The fixed air in this experiment, in which there was ſo great a diminution of the

mon air, it was diminiſhed one-fifth, and the remainder was partly fixed air, and partly inflammable.	common air, muſt have been chiefly, if not wholly, that which was precipitated from the common air. The remainder was ſo far from being inflammable, that it extinguiſhed a candle.
Ib. If the charcoal be made with a very hot fire, capable of melting the crucible, there will be no ſenſible diminution of the air in which it is heated. Charcoal that has been moderately calcined gives no ſign of inflammable air.	This was the caſe in one experiment, but it only proves that *in ſome caſes* air is not ſo eaſily expelled from charcoal as in others. But I have always repreſented the very contrary of theſe reſults as true in general, viz. that a longer continuance of heat, and a greater degree of it, expels more air from wood, and that afterwards the purer will be the phlogiſton that is expelled from it, and conſequently the greater probability there is that the air in which it is heated will be diminiſhed, and not increaſed.
Ib. If the abovementioned proceſs be made over quick-ſilver, and not over water, the air will not be diminiſhed.	This is only the caſe when ſome inflammable air has been expelled from the charcoal in the proceſs.
P. 141, The piece of charcoal employed in this experiment weighed exactly twenty-nine grains.	It weighed exactly two grains, p. 132.

Mr. Lavoiſier, as an introduction to the account of his own experiments, in the work abovementioned, has undertaken to give a pretty full account of all that had been done before him in the ſame way. I hope he has been more exact with reſpect to others, than he has been with reſpect to me.

Sig. Landriani's Account of my Experiments and Obſervations.	*Remarks.*
This writer takes it for granted, through the whole of his work (ſee p. 6, of the Introduction, and p. 3, of the work itſelf) that I conſider the fixed air in the atmoſphere as *un elemento di ſalubrità*, by which I ſuppoſe he means that it is the principle in which its reſpirability conſiſts, or which makes it fit for reſpiration.	All that I have ſaid that could lead to this conſtruction is, perhaps that fixed air, though it certainly kills when it is breathed unmixed, does no ſenſible injury to the lungs when it is mixed with common air; as fire is not noxious *per ſe*, but only *in exceſs*. I have alſo ſhewn that when common air is made noxious by any phlogiſtic proceſs, the fixed air contained in it is precipitated. But though this is a circumſtance that always attends the corruption of air, I never ſuppoſed that the fixed air which it depoſits was the *principle of ſalubrity*. If ſo, I muſt have ſuppoſed that fixed air mixed with air that had been injured by phlogiſtic proceſſes might have reſtored it, which it does not do in the leaſt degree.
P. 24, The diminution of nitrous air by common air, Dr. Prieſtley ſuppoſes, comes from the property that phlogiſton has to contract the dimenſions of bodies: but he gives no ſufficient proof of that ſuppoſition; and though he ſuſpects that this contraction is a real levity communicated to the air by phlogiſton, he has not ventured abſolutely to aſſert it; though chemiſtry	I do not recollect that I have any where ſaid that phlogiſton contracts the dimenſions of bodies. On the contrary, I ſay that air injured by phlogiſton is *ſpecifically lighter* than common air. And what I ſay of the *principle of levity*, p. 267, is, that it is a ſuppoſition I am not willing to have recourſe to, though it would afford an eaſy ſolution of the difficulty.

furniſhes many examples of this ſingular property of phlogiſton.	I might have expreſſed myſelf more ſtrongly; for I never had any faith at all in that doctrine of the principle of levity. On the contrary, as may be ſeen, p. 293, I conſider the difference of weight between a metal and a calx, which has given occaſion to that doctrine, as wholly owing to the fixed air and water imbibed by the latter in the act of calcination.
P. 31, Sig. Landriani repreſents me as ſtating the limit of the diminution of common air by nitrous air to be when *preciſely* two meaſures of the former are mixed with one of the latter; but ſays that he has found that this depends upon the quality of the two kinds of air, and eſpecially on the quantity of phlogiſton contained in the nitrous air.	I have only ſaid, p. 110, that after many trials, I have found that the greateſt diminution is when *about* one-third of nitrous air is mixed with common air, which implies that I was aware of a conſiderable variety in the reſults of ſuch experiments; and the whole of my narrative ſhews that I have conſidered a leſs diminution to ariſe ſometimes from the common air having already more phlogiſton than uſual, and ſometimes from the nitrous air communicating leſs. I frequently ſpeak of different quantities of nitrous air as poſſeſſing very different powers of diminiſhing common air, and ſometimes ſpeak of nitrous air as reduced to a ſtate in which it had no power of diminiſhing common air at all. And as I make it a maxim that common air is diminiſhed and made noxious

	by phlogiſton only, I muſt neceſſarily have conſidered, that nitrous air in this caſe either as not containing phlogiſton, or as not diſpoſed to part with it.
P. 32, Dr. Prieſtley believes that inflammable air becomes reſpirable by agitation in water, becauſe part of the phlogiſton is depoſited in the water, and another part of it remains to ſweeten the acid air, and make it reſpirable.	The former I have ſuppoſed, but I do not remember to have ſaid any thing about the latter. I had, indeed, imagined, that acid air and phlogiſton compoſed inflammable air; and ſuppoſed part of the phlogiſton to be abſorbed by water, when it ceaſes to be inflammable, by means of agitation in water. But I muſt have ſuppoſed, agreeable to the maxim abovementioned, that the phlogiſton which remained muſt have contributed to keep the air in a worſe ſtate than it would be in, if it could be expelled by it, which is the very contrary of what Sig. Landriani has aſcribed to me.

Laſtly, The moſt unaccountable miſtake concerning any of my opinions relating to air, is that I ſhould be ſuppoſed to maintain that *fixed air is a combination of common air and phlogiſton.* Mr. Lavoiſier in Roſier's Journal for May 1775, p. 433, ſays, “ Since common air is changed “ into fixed air by a combination with charcoal, “ it may ſeem natural to conclude that fixed air "is nothing but a combination of common "air and phlogiſton. This opinion is that of "Monſ. Prieſtley."

This, indeed, is the opinion of Dr. Rutherford (*Diſſertatio de aire fixo, p.* 25.) of an Engliſh Chymiſt, who probably had it from him, and that of other philoſophers in this country, who may have adopted it from them; but every thing that is Engliſh is not mine. I have miſtakes enow of my own to anſwer for, and I cannot conceive how any thing that I have ever advanced on the ſubject ſhould have been conſtrued to bear that meaning.

The propoſition which Mr. Lavoiſier advances in the preceding paragraph, on which he ſuppoſes the hypotheſis which he aſcribes to me to have been founded, is not true. I do not know that common air can, by any proceſs, be changed into fixed air; and ſo far am I from ſuppoſing that fixed air is a compound of common air and phlogiſton, that, on the other hand, I have always rather conſidered fixed air as an elementary ſubſtance, and common air as a compound. Moreover, having brought fixed air, by a ſuppoſed union with phlogiſton, to be immiſcible in water, and to have ſome of the properties of common air, I was then inclined to think that fixed air and phlogiſton might make common air, which is the very reverſe of the opinion that Mr. Lavoiſier aſcribes to me. And I do not know that I have ever advanced any thing that comes nearer to that opinion, than this which is expreſsly contrary to it.

In the report made to the Royal Academy of Sciences concerning the abovementioned treatiſe of Mr. Lavoiſier's by Mr. De Trudaine, Mr. Macquer, Mr. Cadet, and the Secretary M. De Fouchy, theſe gentlemen ſay, "Monſ. Prieſtley conſiders fixed air as very "nearly of the ſame ſpecific gravity with the "air of the atmoſphere." Now I have always conſidered fixed air as *conſiderably heavier* than common air. Indeed, I never made any obſervation of my own on that ſubject, having only adopted the concluſion of Mr. Cavendiſh, whoſe diſcovery it was.

I imagine theſe gentlemen have miſtaken what I have ſaid concerning *air injured air by phlogiſton*, which I have ſaid was very nearly of the ſame ſpecific gravity with common air, though rather lighter, for what I have ſaid concerning *fixed air*. And it is poſſible that theſe gentlemen, like Mr. Lavoiſier, may have taken it for granted that theſe two kinds of air are the ſame; though I always ſpeak of them as very different things.

As Dr. Rutherford, when he publiſhed his *Diſſertation on fixed air*, had only heard of my experiments, it would not be worth while to take notice of his miſtakes concerning them; but his treatiſe being tranſlated into French, in Mr. Roſier's Journal, I ſhall juſt obſerve, now that I am upon the ſubject, that he alſo ſuppoſes, p. 25, I had reſtored *fixed air* to a fitneſs for reſpiration by vegetation, whereas it was *air injured by reſpiration or putrefaction*. But this author, as well as many others, makes no difference between theſe two very different kinds of air.

No perſon can lay a leſs ſtreſs upon *opinions*, and more upon *facts*, than I have done, in all my philoſophical writings; and the opinions I have advanced are very few in proportion to the new and important facts that I have diſcovered. I therefore think it rather hard, that thoſe very few opinions ſhould have been ſo groſsly miſrepreſented as they have been.

All the doubts that I have ever entertained with reſpect to the conſtitution of fixed air have little relation to the differences of opinion maintained by others concerning it. I was always inclined to think fixed air to be an acid *ſui generis*, as much as any of the three mineral acids. But as it is the opinion of ſeveral of the moſt eminent chymiſts, that even theſe three acids, as well as all other acids, are only one and the ſame acid, differently modified and combined, and that they are therefore tranſmutable into one another, I have of late conjectured that the fixed air which I have ſometimes produced muſt have been a tranſmutation of the nitrous acid into it, becauſe no ſubſtance employed in the experiments can well be thought to have contained the fixed air, notwithſtanding the air that was produced diſcovered the moſt undeniable ſigns of it, as will have been ſeen in the preceding narrative. Still, however, I do not pretend to have formed any deciſive opinion upon the ſubject. Let the facts be conſidered, and ſpeak for themſelves.

It is maintained by Sig. Landriani, whoſe treatiſe I did not receive till a conſiderable part of this volume was printed off, that fixed air is of a different conſtitution according to the acid by which it is procured from calcareous ſubſtances. He ſays, among other things, p. 48, that the ſalt which is formed by the union of alkaline air with fixed air by oil of vitriol is a true vitriolic ammoniac, and that the ſalt which is formed by alkaline air and the air which is procured by means of ſpirit of nitre has the property of detonating by itſelf, which is known to be peculiar to the nitrous ammoniac. He alſo ſays that fixed air procured by the *vegetable* acids has not the ſame power of reddening the juice of turnſole with that which is procured by the *mineral* acids. Theſe are very remarkable experiments, and deſerve to be repeated, and conſidered with attention. They have led our author to conclude, that all the different kinds of air are, in fact, one and the ſame thing, which has the property of holding in ſolution various bodies, and particularly the acids, ſee p. 33. Accordingly nitrous air, in his opinion, is common air holding in ſolulution a quantity of the nitrous acid overcharged with phlogiſton.

That excellent philoſopher Felice Fontana, in his *Ricerche Fiſiche ſopra l'aria fiſſa*, maintains, that all the acidity of fixed air comes from the oil of vitriol diſſolved in it, and which is ſo intimately united to it, as to be afterwards inſeparable from it; inſomuch that when it has been incorporated with water, and expelled from it again, it carries away all the acid vapour along with it; having all the ſame properties that it had before it was combined with the water. The acid of vitriol, thus attenuated and exalted, by its ſolution in fixed air, is more penetrating, he ſays, and has more medicinal virtues, than the ſame acid diſſolved in water, or adminiſtered in any other form.

Fixed air deprived of this acidity, which is foreign to its nature, he ſuppoſes to be the ſame thing with atmoſpherical air deprived of its peculiar acid by phlogiſtic proceſſes, an acid which he maintains to be altogether different from any acid with which we are acquainted, and which he propoſes to inveſtigate; conceiving this acid to be the great principle of ſalubrity in the atmoſphere. This writer ſays, that he has attempted in vain to make water acidulous by means of fixed air expelled from ſubſtances without the help of other acids, as in putrefaction. But he does not appear to have tried what he could have done with calcareous ſubſtances by heat only.

I take this early opportunity of publiſhing the ſentiments of ſo conſiderable a perſon, though it will appear that they are very different from my own, in order to promote a farther inveſtigation of the ſubject.

Having mentioned the paper of Mr. Lavoiſier's, publiſhed in Mr. Roſier's Journal, I would obſerve, that it appears by it, that, after I left Paris, where I procured the *mercurius calcinatus* abovementioned, and had ſpoken of the experiments that I had made, and that I intended to make with it, he began his experiments upon the ſame ſubſtance, and preſently found what I have called *dephlogiſticated air*, but without inveſtigating the nature of it, and indeed, without being fully appriſed of the degree of its purity. For he had only tried it with one-third of nitrous air, and obſerved that a candle burned in it with more vigour than in common air; and though he ſays it *ſeems to be* more fit for reſpiration than common air, he does not ſay that he had made any trial how long an animal could live in it.

He therefore inferred, as I have ſaid that I myſelf had once done, that this ſubſtance had, during the proceſs of calcination, imbibed atmoſpherical air, not in part, but in whole. But then he extends his concluſion, and, as it appears to me, without any evidence, to all the metallic calces; ſaying that, very probably, they would all of them yield only common air, if, like *mercurius calcinatus*, they could be reduced without addition. For he conſiders the fixed air, which is yielded by moſt of them, to come from the charcoal, made uſe of to revivify the calx. Whereas it will be ſeen, in the courſe of my experiments, that ſeveral of thoſe calces yield fixed air by *heat only*, without any addition of charcoal.

He adds, that ſince common air is changed into fixed air when it is combined with charcoal, it would ſeem natural to conclude, that fixed air is only a combination of common air and phlogiſton (an opinion which, as has been ſeen before, he aſcribes to me) and it is not, he ſays, without probability; but adds, that it is ſo often contradicted by facts, that he deſires philoſophers and chymiſts to ſuſpend their judgments; hoping that it will ſoon be in his power to explain the motives of his doubts. I, for one, am waiting with ſome impatience for this explanation.

Mr. Lavoiſier alſo concludes, from his obſervations, that the air produced by the detonation of nitre and the firing of gunpowder is common air. When he ſees this volume of mine, he will, I doubt not, be convinced of the imperfection of his theory, and of this miſtake, which he has been led into by means of it.

Mr. Lavoiſier, as well as Sig. Landriani, Sig. F. Fontana, and indeed all other writers except myſelf, ſeems to conſider common air (diveſted of the effluvia that float in it, and various ſubſtances that are diſſolved in it, but which are in reality foreign to it) as a ſimple *elementary body*; whereas I have, for a long time, conſidered it as a *compound*; and this notion has been of great ſervice to me in my inquiries.

As a concurrence of unforeſeen and undeſigned circumſtances has favoured me in this inquiry, a like happy concurrence may favour Mr. Lavoiſier in another; and as, in this caſe, truth has been the means of leading him into error, error may, in its turn, lead him into truth. It will have been ſeen, in the courſe of my writings, that both theſe circumſtances have frequently happened to myſelf; and indeed examples of both of them will be found in my firſt ſection concerning this very ſubject of dephlogiſticated air.

It is pleaſant when we can be equally amuſed with our own miſtakes, and thoſe of others. I have voluntarily given others many opportunities of amuſing themſelves with mine, when it was entirely in my power to have concealed them. But I was determined to ſhew how little *myſtery* there really is in the buſineſs of experimental philoſophy, and with how little *ſagacity*, or even *deſign*, diſcoveries (which ſome perſons are pleaſed to conſider as great and wonderful things) have been made.

SECTION XVII.

Experiments relating to ſome of the preceding Sections made ſince they were printed off.

Having had an opportunity of making a few farther experiments relating to ſome kinds of air mentioned in this treatiſe, after the ſections relating to them were printed off, I have thought it would be better to ſubjoin an account of them in this place, rather than defer it to another publication.

1. *Of the vitriolic and vegetable acid airs.*

It will be ſeen, by comparing the firſt and ſecond ſections of this volume, that there is a remarkable reſemblance between the vitriolic and vegetable acid airs, and I have ſince obſerved other circumſtances of reſemblance.

The electric ſpark taken in vegetable acid air produces the very ſame effect as in the vitriolic acid air, tinging the glaſs tube in which it is contained with a deep brown, or black colour. I took about fifty exploſions of a common jar in a ſmall quantity of it, after which water imbibed almoſt the whole of it.

It is remarkable that the glaſs becomes almoſt as deeply tinged as it can be made by the experiment, after a very few of the exploſions.

I alſo obſerved the ſame remarkable effect of putting ſmall glaſs tubes half filled with water into vitriolic acid air, that I have deſcribed p. 26, as obſerved in the vegetable acid air, viz. that if a little air be left at the bottom of the tube, it will ſwell, and drive out all the water. The reaſon of this appearance I believe to be, that the water, being preſently ſaturated with this acid air, tranſmits it to the common air in the tube; which, receiving a continual increaſe of bulk from this ſource, at length expels all the water.

I obſerved this appearance when I put the tubes, thus partially filled with water, into that air which I had expelled from the water that had been ſaturated with the fluor acid air; which is another argument of the identity of this acid with the vitriolic.

Water is ſoon impregnated with vitriolic acid air, but has little power of retaining it; ſo that the ſmell of the water ſo impregnated is the moſt pungent that can be conceived, and if it ſtands expoſed to the common air, the acid air, in a great meaſure, preſently quits it. Alſo the leaſt agitation of the water promotes the ſeparation of the air from it.

I have obſerved, p. 10, that a mixture of vitriolic acid air injures common air, and that the effluvium of the concentrated vegetable acid has the ſame effect. I have ſince found that a mixture of the vegetable acid air itſelf does ſo to. Two meaſures of this mixture, and one of nitrous air occupied the ſpace of two meaſures.

The only real difference between the vegetable and the vitriolic acid air (beſides the *ſmell* of them, in which reſpect the difference is remarkable enough) that I have obſerved, is, that, whereas the vitriolic acid air, as well as all the other acid airs with which I am acquainted, deepens the colour of olive oil, an impregnation with vegetable acid air makes it more colourleſs. In one experiment, however, vegetable acid air gave a yellowiſh tinge to oil of turpentine, which is an effect that vitriolic acid air has upon it; though, upon another occaſion, the reſult of this experiment was different, and I have not leiſure at preſent to examine whence this difference aroſe.

As Dr. Higgins has informed me, that oil of vitriol was employed in preparing the concentrated vinegar that I made uſe of for the production of vegetable acid air, I think it poſſible that the air which I expelled from it may have been, in part, of the vitriolic kind; but I do not know of any other vegetable acid liquor that will yield air; at leaſt in a quantity ſufficient for any experiments. I tried *radical vinegar of the cryſtals of verdigris rectified*, which was recommended to me, and made for me, by Mr. Woulfe, and alſo concentrated acid made from *ſal diureticus*, by Mr. Godfrey; but neither of theſe acid liquors, though the ſmell of them was extremely pungent, yielded any air by heat.

The common air expelled from the phial by the ſteam of this vinegar, mixed with whatever acid vapour might come over along with it, I examined, after letting it reſt upon quickſilver a whole night, and I found it not to differ from common air.

When, however, I tried this experiment with air that had lodged on the ſurface of oil of vitriol, into which I had put ſome *ſal diureticus*, and which did yield a little air, the common air did appear to be injured by the mixture, as in the preceding experiment of the mixture of common and vegetable acid air. But then oil of vitriol being employed in this experiment, as well as in the preparation of the concentrated vinegar abovementioned, it is liable to the ſame objection; the acid of vitriol being, perhaps, volatilized by ſome ſmall portion of phlogiſton.

2. *Of dephlogiſticated air.*

I have obſerved a great variety in the reſults of the experiments for the production of dephlogiſticated air, both with reſpect to the quantity, and the quality of it, eſpecially as mixed with a greater or leſs proportion of fixed air. From the following experiments it will appear that the quantity of dephlogiſticated air depends upon the quantity of the ſpirit of nitre made uſe of in the proceſs, the quantity of fixed air being nearly the ſame in all the caſes.

From an ounce of red lead, heated in a gun-barrel, I got about an ounce meaſure of air, which all together was worſe than common air; an effect which I attribute, in a great meaſure, to phlogiſton diſcharged from the iron. The production of air in this caſe was very ſlow.

From an ounce meaſure of the ſame red lead, diluted with half ſpirit of nitre and half water, I got twelve ounce meaſures of air, the laſt produce of which was highly nitrous. Half of this quantity was abſorbed by water, and the remainder was twice as good as common air.

From an ounce of the ſame red lead, diluted with the ſame ſpirit of nitre, without water, I got, by the ſame treatment, about thirty ounce meaſures of air, about one-eighth of which was abſorbed by water, while the reſt was highly dephlogiſticated.

From the ſame quantity of red lead, moiſtened with twice the quantity of the ſame ſpirit of nitre, I got about ſixty ounce meaſures of air, a very ſmall part of which was abſorbed by water, and the reſt was as highly dephlogiſticated as that in the laſt experiment.

The produce of air was quicker, with the ſame degree of heat, in proportion as the quantity produced was greater; and in the laſt proceſs the air was very red in the inſide of the veſſel that received it, for a conſiderable time.

3. *Of the effect of the nitrous acid on common air.*

I have ſhewn, in a variety of experiments, that the fumes of ſpirit of nitre injure common air. I have found the ſame to be the effect of the effluvia of *nitrous ether*. For the air which had been confined about a week, in a bottle in which a quantity of nitrous ether had been kept, was ſo much injured, that two meaſures of it, and one of nitrous air, occupied the ſpace of 2 ½ meaſures. As I let a good deal of common air into the phial, at the ſame time that (not chuſing to loſe it) I poured the ether out of it into another phial, I conclude that the air in the phial was almoſt perfectly noxious.

I have more than once expreſſed an earneſt wiſh that I could meet with any fluid ſubſtance that was not affected with the nitrous acid, as this would give me an opportunity of confining the nitrous acid air, in order to make experiments upon it, as I have done upon other acid airs; and I almoſt flatter myſelf that I have accidentally met with one that will anſwer my purpoſe. It will be ſeen, p. 156, that *hog's lard* is very little affected with boiling ſpirit of nitre. Upon finding this, I immediately endeavoured, by means of heat, to expel nitrous acid air from a quantity of ſtrong ſpirit of nitre; thinking that it might be confined in a veſſel filled with melted hog's lard, as the other acid airs had been confined by quickſilver. But though I made the ſpirit of nitre boil a long time, I got nothing from it but the common air which had lodged on the ſurface of the acid, and which I found to be ſo far injured by the proceſs, that two meaſures of it, and one of nitrous air, occupied the ſpace of 2 $\frac{1}{2}$ meaſures. I ſhall try whether I cannot have better ſucceſs with ſome other *animal oil*, as the *ſpermaceti oil*, &c. making ſome other varieties in the proceſs.

4. *Of fixed air.*

I have made an obſervation, p. 220, of the degree of the purity of the reſiduum of fixed air which had been wholly contained in water, at a time when it hardly made lime water turbid. I afterwards kept the ſame reſiduum, waſhing it ſeveral times in lime water, till it had no effect upon it whatever. At this time two meaſures of it, and one of nitrous air, occupied the ſpace of 2 $\frac{1}{2}$ meaſures. In fact, therefore, the reſiduum of fixed air is, in the main, the ſame thing with phlogiſticated common air; though in this caſe it was meliorated by ſo much waſhing in water. To the ſame ſtate alſo are all kinds of air whatever, and even nitrous air itſelf, reduced, by much agitation in water. This is a remarkable fact, and may furniſh matter for ſpeculation.

To my ſhort account of my obſervations on the Seltzer ſpring, and the other mineral water near Mentz, p. 226, I would add, that the bottom of both of them, and alſo of the current of water that ran from them, was tinged red with ochre, ſo that it is evident they both contain iron.

5. *Of the impregnation of water with fixed air.*

I find I have expreſſed myſelf too ſtrongly with reſpect to the evidence of other perſons having had in view any ſcheme of the impregnation of water with fixed air, before the publication of my pamphlet on that ſubject; on the ſuppoſition that, if ſuch evidence had exiſted, it would have found its way to the public by this time.

My ingenious correſpondent Mr. Bewly, on ſeeing that part of the work, informs me, that he had not read Dr. Brownrigg's paper half through, before he expected that the *ſyntheſis* would follow the *analyſis*, and that finding his author intirely ſilent on the ſubject, he imme-

diately

diately went to work himſelf, and in a common phial effected the impregnation, by fixed air ſet looſe from ſalt of tartar; and though he had but an imperfect kind of an apparatus, he ſays he has occaſionally regaled himſelf and his friends with ſmall potations of artificial Pyrmont water, ever ſince the publication of Dr. Brownrigg's paper.

I cannot help obſerving on this occaſion, as on many others, that it is much to be regretted, that perſons of a philoſophical turn ſhould not be more diſpoſed to communicate their diſcoveries to the public. In this caſe, however, it will be ſeen that I am not myſelf without blame, as I made no publication on the ſubject till ſome years after I had effected this impregnation, though Mr. Bewly, I find, had done the ſame thing a conſiderable time before me.

6. *Of the uſe of terms.*

I am ſorry to find, that notwithſtanding what I ſaid, in the preface to my former volume, on my choice of the term *air*, as applied to the *nitrous*, *acid*, and *alkaline* principles exhibited in that form, ſome perſons are either ſo weak, or ſo captious, as not to be ſatisfied.

No perſon was ever more temperate, or more cautious, than I have been in the introduction of *new terms*, conſidering the number of *new facts* that I have diſcovered. It was with great heſitation, though compelled by neceſſity, that I did it at all, generally with the advice of my moſt judicious friends, and always adopting ſuch as were analogous to others in eſtabliſhed uſe. Thus when I found the terms *common or atmoſpherical air*, *fixed air*, and *inflammable air*, uſed by all philoſophers, and no perſon whatever had objected to them, it was certainly natural for me to continue to apply the term *air* to other *elaſtic tranſparent fluids*, not condenſable by cold, and to diſtinguiſh them by other appellations, drawn from the peculiar circumſtances of their production, as *nitrous air*, *acid air*, *alkaline air*, *phlogiſticated* and *dephlogiſticated air*; uſing the term *air* as expreſſive of the mere *form* in which a ſubſtance is exhibited, without any conſideration of its being ſimple or compound.

They who chuſe to apply the term air to a *ſubſtance*, and not to a *form*, are certainly at full liberty ſo to do, if they pleaſe; and provided we underſtand one another, no inconvenience will reſult from our uſe of a different language. But then the ſame perſons ſhould be uniform in their objections and practice, and call nothing by the name of *air* that they do not believe to conſiſt of that one *elementary ſubſtance* to which they profeſs to appropriate the term. I will add alſo, that ſuch perſons will do well to prove that there *is* ſuch an elementary ſubſtance, and to reconcile the facts that I have diſcovered with that hypotheſis. The language that I adopt implies no attachment to any hypotheſis whatever, and may ſtill be uſed though I ſhould change my opinion on that ſubject; which is certainly a very great advantage in philoſophical language.

THE APPENDIX.

NUMBER I.

EXPERIMENTS and OBSERVATIONS *relating to ſome of the Chemical Properties of the Fluid, commonly called* FIXED AIR; *and tending to prove that it is merely the* VAPOUR *of a particular* ACID. *In two Letters to the Reverend Dr.* Prieſtley: *By* William Bewly.

ANY ſucceſsful inveſtigation of that part of philoſophy, in which you have lately made ſo extenſive and rapid a progreſs, cannot be communicated to the publick any where with ſo much propriety, and advantage, as in the company of thoſe ſingular and important diſcoveries which will be given in the new Volume of your *Obſervations*, now in the preſs. With great pleaſure, therefore, I comply with your late requeſt, to tranſmit to you the particulars of my Obſervations on *Mephitic* or *Fixed Air*; the general reſults of which I formerly communicated to you. They tend, if I do not deceive myſelf, to throw a new and juſt light on the real nature and chemical properties of that fluid; the extenſive diffuſion of which throughout the univerſe, where it forms a conſtituent principle of almoſt all known bodies, renders it a ſubject deſerving of an accurate and minute inveſtigation.

In the preſent letter I ſhall principally confine myſelf to thoſe Obſervations only which firſt convinced me of the exiſtence of an acid *in* fixed air:—— a point which has been conteſted, or, at leaſt, left dubious, by other inquirers. The experiments which I ſhall relate in a ſubſequent letter will, I expect, ſatisfactorily evince, that this acid is not a ſubſtance extrinſecal to fixed air, or caſually floating in it, and ſeparable from it; but, on the contrary, that it is a neceſſarily conſtituent principle of this fluid; and even that fixed air itſelf is no other than this very acid; or, in other words, that it is a peculiar and diſtinct acid ſpirit, *ſui generis*, which, on its being expelled, by the power of a ſuperior acid, or the force of fire, from the various earths, ſalts, &c. with which it is combined, inſtantly aſſumes the form of an elaſtic vapour, greatly reſembling common air; which form it permanently retains, till it meets with any of thoſe numerous bodies which have an affinity to it, and which have been deprived of, or are not already ſaturated with it. By theſe bodies this acid vapour is *condenſed*, or reduced into a *liquid* or fixed ſtate; in which ſtate it combines with them, in a manner in no reſpect different from that in which the vitriolic or any other acid is united with the various ſalts, earths, or other ſubſtances, with which they form neutral compounds— Such, at leaſt, is the ſyſtem which I have been naturally led to deduce from the following experiments.

Even the bare preſence of an acid, *in* fixed air, has, as I have already obſerved, been doubted of. So lately even as the laſt year Dr. Brownrigg *, to whom this new branch of Chemical philoſophy is ſo highly indebted, obſerved, that though Mephitic air, imparts to the waters impregnated with it a briſk and pungent taſte, which has uſually been ſtiled *ſubacid*; yet it differs from all acid ſpirits in not ſtriking a red colour with the blue tinctures of vegetables; adding, that not only no change of this kind could be obſerved to have been effected by it, in the numerous experiments made by himſelf and ſeveral other gentlemen; but likewiſe that he had "for ſeveral days ſuſpended "pieces of linen, that had been dyed blue with freſh "juice of violets, in the mephitic air of *Spa* water, "and alſo in that of chalk; and when the linen was "taken out of the ſaid air, did not perceive its blue "colour in any wiſe changed, although the ſame "pieces of dyed linen were inſtantly turned of a green "colour, when expoſed to the fumes of the ſpirit of "hartſhorn."——"Whether therefore," he adds, "and under what relations, this aerio-ſaline ſpirit "may merit the title of an *acid*, I leave to the determination of others."

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* Phil. Tranſ. vol. 64. part 2. for the year 1774. p. 369.

In the *Appendix* to your former Volume, your ingenious correſpondent, Mr. Hey †, has likewiſe ſhewn that water impregnated with fixed air, produced no change of colour in the ſyrup of violets; and that it did not efferveſce with either the fixed or the volatile alcali.——The fact is, that *fixed air* is ſo rare a *vapour*, and the *Mephitic Acid*, as I ſhall already venture to call it, is ſo greatly diluted in water, which is even ſaturated with it, that many of the blue juices reſiſt its action upon them; while others, more ſenſible teſts of acidity (ſuch as infuſions of Litmus, *Cyanus*, or Corn-flower, and a few others) readily anounce its acid quality.——As to its not producing an efferveſcence with alcalis, it will appear from the following experiments, that ſuch efferveſcence is, from the very nature of the thing, impoſſible in the preſent caſe, in which the very contrary of an effervefcence muſt take place. In all other caſes, when an acid is added to a mild alkali, the *mephitic acid*, as being the leaſt powerful of all the acids, is *expelled*, in its ſtate of *vapour*, or in elaſtic bubbles, which conſtitute the appearance called an effervefcence; whereas when the mephitic acid itſelf is added to an alcali, it is *condenſed*, and ſilently abſorbed in it.

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† Experiments and Obſervations, &c. p. 288. 1ſt edit.

It may be neceſſary to premiſe that, in ſeveral of the following experiments, I found it moſt convenient, as well as productive of greater accuracy and expedition, to take the inverted phial out of the baſon, after every freſh introduction of fixed air, for the purpoſe of agitating more freely the liquor contained in it; and that I took care to uſe a baſon or cup of a very ſmall diameter, and which contained a very ſmall quantity of fluid; in order to guard, as much as poſſible, againſt diſſipation of the fixed air, during the proceſs. It may be proper likewiſe to obſerve, that I may not incur a ſuſpicion of plagiariſm, that ſome ſcattered hints, relative to a few of the following Obſervations, have been formerly inſerted by me in a certain anonymous publication.

The experiment, by which I firſt detected the preſence of an acid in fixed air, ſome years ago, is as follows. I have repeated and diverſified it on the preſent occaſion, and with the ſame event.

EXPERIMENT I.

Having accurately adapted, to the mouth of a phial containing ſpirit of vitriol, a cork, in which a glaſs tube was inſerted, which was drawn out at its farther extremity, ſo as to terminate in a bore nearly capillary;

lary; and having thrown into it ſome ſalt of tartar, I haſtily applied the cork, and inſtantly preſented, cloſe to the end of the tube, a piece of a particular kind of blue paper, uſed for the covers of pamphlets, as well as other pieces of paper tinged blue with the ſcrapings of radiſhes. When the effervеſcence was briſk, and proper expedition was uſed, the mephitic vapour ruſhing out, undiluted with common air, and in a denſe and ſometimes viſible column, inſtantly changed thoſe parts of the blue paper, towards which it was directed, of a bright red colour. On bringing the tongue likewiſe to the end of the tube, the ſenſation of acidity was very ſenſible.

EXPERIMENT II.

The ſucceſs of the preceding experiment wholly depends on the denſity and velocity of the mephitic blaſt. Having afterwards cauſed the fixed air to paſs through moiſt alkaline ſalt introduced into the tube, it now only, in general, weakened or diſcharged the colour of the blue paper. This effect I was at firſt inclined to attribute to the vitriolic acid, phlogiſticated or volatiliſed, which is known to act in this manner on various coloured ſubſtances: but from the following experiments it may be inferred, that the change was produced by the mephitic acid's being in part neutraliſed, and conſequently diminiſhed in quantity, in its paſſage through the alkali; ſo that the remaining vapour, though as acid as before, was in too rare a ſtate, and had not *momentum* ſufficient to produce the red colour.

EXPERIMENT III.

Six ounces of a weak infuſion of Litmus in water, being impregnated with two or three ounces of fixed air, had its blue changed to a red or pink colour.—A weak and nearly colourleſs infuſion of the petals of the corn-flower, as well as infuſions of two or three other blue field flowers, acquired likewiſe a ſlight reddiſh tinge, on being even weakly impregnated with fixed air.

EXPERIMENT IV.

Having prepared a *Hepar Sulphuris*, in the liquid way, and in which the alcali was fully ſaturated with the ſulphur; I diluted a part of it with rain water, and added to it a few ounces of water ſaturated with fixed air. The impregnated water produced the effects which are known to follow the addition of any acid to an alcaline ſolution of ſulphur. The liquor became milky and opaque; and after ſome time part of the ſulphur was precipitated: doubtleſs by the action of the mephitic acid, which joined itſelf to the alcali, and thereby diſengaged a proportional part of the ſulphur before combined with it.

EXPERIMENT V.

The ſucceſſive action of the acid in fixed air on ſuch of the blue vegetable juices as it changes to red, or its gradual entrance into water, is very pleaſingly exhibited by filling a phial, which has a ſmall hole drilled near its bottom, with an infuſion of Litmus, and then introducing into its neck a perforated cork, to which is fixed a bladder, containing fixed air. Preſſing the bladder till the liquor deſcends to the broad part of the phial, the perforation is to be ſtopped; and the infuſion being ſuffered to remain perfectly at reſt, the gradual entrance of the fixed air into it (or rather the condenſation of the mephitic acid) will be rendered viſible, by the ſucceſſive change of colour in the liquor, from the ſurface downwards, from blue to red.—This experiment may be diverſified by employing the proceſſes indicated in Experiment 7th.

The fixed air employed in the preceding experiments was generally procured from ſalt of tartar, by means of the vitriolic acid. The greater part however of theſe and the following experiments were repeated with fixed air, obtained from the following ſubſtances, or combinations; from which I rejected the nitrous and marine acids, for obvious reaſons, particularly on account of their volatility. I tried the fixed air procured from the vitriolic acid added to chalk, and even from the ſame acid and mild *volatile* alcali; that ſlowly obtained from fixed alcali and cream of tartar; and even that which riſes from wort in the act of fermentation. The ſame ſigns of acidity were exhibited by the fixed air obtained from all of them.

In all theſe proceſſes, however, an acid, mineral or vegetable, might be ſuſpected to have been concerned as an ingredient in the proceſs, in the production of the effects above aſcribed to the fixed air. In the following experiment, therefore, I uſed fixed air expelled from a body, without the intervention of an acid, and merely by the force of fire.

EXPERIMENT VI.

A phial, to the mouth of which a glaſs tube had been joined, by means of the blow pipe, was filled with Magneſia, the perfect purity of which I had previouſly aſcertained. Having placed it in ſand contained in a crucible, which was ſet upon the fire, the air proceeding from it was ſucceſſively received into ſmall phials filled with infuſion of Litmus. The firſt ounce, which came over even before the Magneſia could be thoroughly heated, though neceſſarily mixed with common air, tinged the infuſion of a red colour. The ſubſequent produce (which came over to the amount of eight ounces, when the apparatus was accidentally broken) continued to exhibit the ſame ſigns of acidity, as were given by the fixed air, expelled from alkaline ſalts and earths, by means of acids. It poſſeſſed likewiſe the other properties of the laſt mentioned fixed air, which will be related in the next, and ſome of the following experiments.

Finding the acid in fixed air not ſtrong enough, or ſufficiently concentrated, to act ſenſibly on the generality of the blue vegetable juices; I conceived, that by the following method the greater part of them might nevertheleſs be made to bear teſtimony to its acid quality.

EXPERIMENT VII.

Having diluted ſome ſyrup of violets with water, and prepared different infuſions of ſuch of the blue vegetable flowers as were not changed red by fixed air; there were added to each of them a few drops of a ſolution of fixed alcali, ſufficient to turn them to a *green* colour. A few drops likewiſe of the ſame ſolution were added to an infuſion of Litmus. On impregnating theſe different liquors with fixed air, (from ſpirit of vitriol and chalk) the infuſion of Litmus was changed red as before; and the green colour given to the other blue infuſions, by the alkaline ſalt, was deſtroyed by the fixed air:—an evident proof, that the *alcali*, by which the green colour had been produced, had been *neutraliſed* by an *acid*.

The ſame effects were produced by impregnating the infuſions with the fixed air procured, in the preceding experiment, from Magneſia, by ſimple calcination;—as likewiſe from chalk calcined in a tobacco-pipe, and afterwards in a gun-barrel: tho' the greater part of the produce, in this laſt proceſs, was, as you have already noticed, inſoluble in water, and inflammable.

All theſe experiments, and others of a ſimilar nature, proved only that an acid exiſted *in* fixed air. This laſt however induced me to extend my views, and ſuggeſted a ſeries of experiments, which led me to the concluſion announced in the beginning of this letter;—that fixed air, when pure, and from whatever ſubſtance obtained, is only a peculiar acid, in a ſtate of vapour; which particular modification it aſſumes on its expulſion from various bodies, by the power of a ſuperior acid taking its place; or by that of fire. Theſe experiments, which are as ſimple, as they appear to me to be deciſive, ſhall be the ſubject of another letter.

WILLIAM BEWLY.

Great Maſſingham, Norfolk, Sept. 23, 1775.

LETTER

LETTER II.

Great Maſſingham, Sept. 27, 1775.

I have hitherto attempted merely to aſcertain the exiſtence of *an* acid in fixed air. The avowed purpoſe of the preſent letter is no leſs than that of introducing a new ſubject into the tribe of acids; and of ſhewing that the aforeſaid *acid* is, in fact, the very ſubſtance denominated fixed *air*. The following experiments will at leaſt, I flatter myſelf, deciſively prove that it is eſſential to the conſtituence of that fluid; and that it cannot be deprived of it, without ceaſing to be fixed air.

Apprehending that, if the acid detected in fixed air were only a foreign or contingent principle, caſually floating in this fluid, it might be deprived of this adventitious ſubſtance, by means of an alcaline ſalt, and yet ſtill retain its other diſtinguiſhing characteriſtics, of elaſticity, abſorption in water, &c. I purſued the hint ſuggeſted to me by the event of the laſt experiment, by trying whether I could not diveſt it of this ſuppoſed adventitious acid, and thereby procure and examine it in a ſtate of purity. For this purpoſe I firſt made the following experiment;

EXPERIMENT VIII.

Filling a two ounce phial with a ſtrong ſolution of *mild* fixed alcali, and putting into a cup of a ſmall diameter a very little quantity of the ſame ſolution, barely ſufficient to allow me to immerge the neck of the phial into it, without ſuffering the common air to enter; I found that, on throwing about an ounce of fixed air into it repeatedly, and alternately taking the phial out of the cup, and agitating its contents, the fixed air *totally* * diſappeared each time; and, upon the whole, in ſuch quantities, as could not eaſily be accounted for, on any other hypotheſis, than that it was merely the vapour, or the elaſtic fumes, of an *acid* ſpirit, condenſed, and combined with an *alcali*. Several ounce meaſures of fixed air were thus made to diſappear ſucceſſively; and I, at length, diſcontinued the proceſs, through mere laſſitude.

If the alcali had only laid hold of an extraneous acid floating in fixed air, it might have been expected, that the aerial ſubſtance, or vehicle, which contained it, might have remained, with only ſome ſlight diminution of its bulk: but on every freſh introduction of fixed air, nearly the *whole* of it vaniſhed; and the *alcali*, which was *mild*, evidently appeared to act, not as an abſorbent of a ſuppoſed aerial ſubſtance, but as an *Antacid*.

I next made the following experiment, with water, in which I diſſolved a ſmall and known quantity of mild alkaline ſalt; in order to determine how much of the alcali a given quantity of fixed air was capable of neutraliſing.

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* When I uſe this expreſſion, or others of a ſimilar import, here and elſewhere, I ſcarce think it neceſſary to obſerve, that a very ſmall *reſiduum* was left after each trial; the ſpace occupied by which I uſually filled up, for the ſake of expedition, from the liquor in the cup. I never collected theſe *reſidua*; which I conſider as *impurities*, conſiſting, in part at leaſt, of common air; from which, and inflammable air, it is ſcarce poſſible to procure fixed air perfectly free. Whatever they may be, they certainly are not the ſubſtance we uſually deſign by the name of fixed air.

Expe-

EXPERIMENT IX.

I impregnated five ounces of Well-water with fixed air, till it would receive no more. I could not make it abſorb more than about four ounces. I then added to the water 20 grains of ſalt of tartar, previouſly diſſolved in a ſmall quantity of water; and immerging the mouth of the phial into a ſmall cup, containing water, I threw up into it about half its bulk of fixed air. On agitating the liquor, and again immerging the mouth of the phial, and then ſlowly withdrawing my finger, the liquor in the cup, though the greateſt part of it had been before *ſaturated* with fixed air, ruſhed up into the phial, with nearly as much violence as if a *vacuum* had been formed in the upper part of it.—The effect naturally reminded me of the condenſation of ſteam or vapour in the fire engine; and is ſcarcely to be accounted for, without conſidering it as proceeding from a ſimilar cauſe, or conceiving the included fixed air, as a greatly expanded and elaſtic vapour of an *acid* ſpirit, ſuddenly *condenſed*, and immenſely reduced in its dimenſions, *qua acid*, by the action of the *alcali*.—Freſh portions of fixed air, introduced into the alcaline ſolution, ſucceſſively diſappeared; and, upon the whole, in conſequence of the addition of only theſe twenty grains of alcaline ſalt, the water received or condenſed about ſeven or eight additional ounces of fixed air.

EXPERIMENT X.

That I might ſee the progreſs of the neutraliſation, I diverſified the preceding experiment, by colouring water with Litmus, ſyrup of violets, and other blue infuſions; and by the change of colour induced, was enabled to ſee the action of the *mephitic acid* on the different alcaliſed liquors; and in the infuſion of Litmus particularly, could perceive the final predominance of the acid, as in the 7th experiment, by means of the red colour given by it to the liquor.

Should the foregoing evidence for the exiſtence of the *mephitic acid*, founded on the viſible changes of colour produced by it, be queſtioned; it is corroborated, and indeed rendered unqueſtionable, by the teſtimony of another ſenſe, in the following experiment.

EXPERIMENT XI.

The laſt experiment was repeated, with a larger proportion of alkaline ſalt; each ounce of water now containing ſix grains of alkali. The ſolution had in a high degree the well-known acrid, urinous, and abominable taſte of the alkaline ſalt. Taſting it, at different times during the courſe of the impregnation, the acrid and lixivial flavour was found to be gradually diminiſhed, in proportion as the fixed air was combined with it. Towards the end, the alcaline and urinous flavour was completely deſtroyed, by the action of the mephitic acid; and when the alcali was perfectly neutraliſed, the ſolution, which was coloured with Litmus, on being well agitated with freſh portions of fixed air, received ſtill more of that fluid; at the same time it became red, and its taſte was now ſimply, and not diſagreeably, ſaline, and even *ſubacid*.

From theſe and ſome other experiments, I eſtimated, that an ounce of fixed air, or *acid mephitic vapour*, will neutraliſe between three and four grains of *mild* fixed alcali; or perhaps ſomewhat more. It was not indeed eaſy, by this method, to aſcertain the exact quantity. Part of the mephitic acid was doubtleſs neutraliſed, even in its paſſage, in ſmall bubbles, (as was the caſe in my experiments) through the alcaliſed liquor. On the other hand, it is difficult to know whether, and how far, this loſs by *condenſation*, was counterbalanced, or more than counterbalanced, by the *diſſipation* at the ſurface of the liquor in the baſon.

EXPERIMENT XII.

Effects ſimilar to thoſe related in the preceding experiment were produced, on adding the *volatile* alcali to water, and likewiſe the *foſſil* alcali; but, as might be expected, in a leſs degree. It is ſuppoſed, that the latter owes the principal properties which diſtinguiſh it from the fixed vegetable alcali, to its containing a larger proportion of fixed air. As it is likewiſe frequently impure, if it ſhould contain any of the marine acid capable of being diſengaged from it; that acid, as ſuperior to the mephitic, muſt contribute to prevent ſo large a portion of the latter from entering into the alkaline ſolution, as would be received when the pure vegetable alcali is employed.

EXPERIMENT XIII.

Having thus obtained a perfectly new *neutral ſalt*, (though in a ſtate of ſolution) I was deſirous of aſcertaining ſome of its chemical qualities; and particularly of trying whether fixed air, after having been neutraliſed by an alcali, might not be expelled from it by means of fire, and come over poſſeſſed of its acid quality.

I took therefore ſome of the produce of the 11th Experiment, and firſt neutraliſed the ſuperabundant mephitic acid, by dropping in *Lixivium tartari* till the ſolution loſt its red colour, and became blue. With this liquor I nearly filled a phial, to which a bent tube was accurately adapted, and well ſecured with very ſtiff cement. Putting it into a pan of water, placed on burning coals, I ſet a phial, filled with infuſion of Litmus, over the extremity of the tube, which was immerſed in a baſon of water. I was ſurpriſed to find that no ſenſible part of the large quantity of mephitic air, or acid vapour, contained in the ſolution in a condenſed ſtate, was expelled from the alkali, though the water in the pan was made to boil violently: and yet innumerable ſmall bubbles, probably the mere vapour of the heated liquor, were perceived to aſcend from the bottom of the phial. A very ſmall portion of air indeed came over, at the beginning of the proceſs, into the inverted phial: but no part of it was abſorbed by the infuſion; nor could any change of colour be produced in the latter by agitation; neither did the ſaline ſolution, though ſo long ſubjected nearly to a boiling heat, exhibit, when cold, any taſte of the alcali contained in it. To diſcover whether the apparatus might not have deceived me, I repeated the experiment in the ſame bottle with pure water, ſaturated ſolely with fixed air; the greateſt part of which came freely over, and aſcended into the inverted phial.

EXPERIMENT XIV.

Finding the mephitic acid thus reſiſt nearly a boiling heat, when combined with the fixed alcali, but ſhut out from all communication with the common air; I was deſirous of trying whether the neutral ſalt, formed of theſe two ſubſtances, might not poſſibly be procured in a concrete or cryſtalline form. But, on expoſing different coloured ſolutions, the product of the 11th Experiment, in broad plates, to the common air, in a warm room; the early change of their colour ſoon convinced me—(though the great quantity of the mephitic acid, which alcaline ſalts contain in their common ſtate, ſtrongly adheres to them even in a conſiderable heat—) that the acid, ſuperadded to that before combined with them, has a much greater affinity to atmoſpherical air, than to fixed alcali.—In a few hours, the flight of the mephitic acid was ſenſible to the taſte; the infuſions becoming gradually more ſenſibly alcaline. Having uſed only ſmall quantities, I cannot ſpeak preciſely as to the particular nature of the ſalt left after the evaporation of the greater part of the water. In ſome of the plates, very ſmall cryſtals were formed; but the greateſt part of the ſolution continued deliqueſcent.—On the whole, the remaining ſalt did not appear to be *foſſil alcali.*

EXPERIMENT XV.

Finding that fixed air acted as an acid, in perfectly neutraliſing alcaline ſalts, I was naturally led, from analogy, to expect that it might likewiſe diſſolve calcareous earths. On adding the fineſt powder of common chalk to water, in a ſufficient quantity to render it milky and opaque, I found, that on repeatedly and forcibly agitating the liquor with freſh portions of fixed air, its milkineſs and opacity gradually diſappeared. The whole of the earth was at length perfectly diſſolved, and the water became tranſparent. —Pure magneſia was diſſolved in the ſame manner.

When I tried this experiment, I did not recollect one made by the Hon. Mr. Cavendiſh, in his obſervations on what he calls the *unneutraliſed* earth in Rathbone-place water, and other waters *; by which he means an earth not diſſolved or ſaturated by any of the known mineral acids, but ſuſpended in water by an additional proportion of fixed air. The preſent experiment ſhews that it is neutraliſed, or diſſolved, at leaſt, by the mephitic acid.

It is remarkable, as he obſerves, that *pure*, or calcined calcareous earth, which is ſoluble in water, ſhould, on being impregnated with fixed air, become totally inſoluble in that fluid; and that by adding a ſtill further portion of fixed air, it ſhould be again rendered capable of being ſuſpended in water. Conſidering fixed air as an *acid*, the ſingularity in a great meaſure diſappears. More than one inſtance in chemiſtry occurs to me that reſemble the foregoing.—Thus *calomel*, or mercury combined with the marine acid, is almoſt totally inſoluble in water (one grain requiring near 2000 times its weight of boiling water to diſſolve it); but *mercury ſublimate*, or mercury combined with a ſtill larger portion of the ſame acid, is very readily ſoluble in the ſame liquid ‡.

Since I wrote what goes before, the idea of an experiment occurred to me, which I immediately executed; and the reſult of which, though it comes in here rather out of its place, is too material and deciſive to be omitted.

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* Phil. Tranſ. Vol. 57. p. 104.

‡ See Baume's *Chymie. Experimentale*, &c. Tom. 2. p. 428, &c.

EXPERIMENT XV.

Recollecting that you had obtained fixed air, by means of fire, from volatile alcaline ſalts, I diſſolved ſome of the volatile ſalt of *Sal ammoniac* in water, with which I nearly filled a phial fitted up with a bent tube, which I ſet on the fire in a pan of water. The fixed air which was expelled from this *alcaline* ſalt, *without employing any other medium than heat*, exhibited the very ſame *phenomena* with that procured by the intervention of foreign acids. Although much volatile alcali muſt have come over with it, and neutraliſed a conſiderable part of it; yet the *mephitic acid* was ſo much more abundant, as not only to neutraliſe the alcaline vapours that roſe along with it, but to be predominant in the coloured infuſion into which it was received. This liquor was ſo far *acidulated* with it, as to become of a bright red; and it required a ſenſible quantity of fixed alcali to reſtore its blue colour, and neutraliſe it.

EXPERIMENT XVI.

In conſequence of the reſult of this experiment, (though I had before found [EXPERIMENT XII.] that fixed air could not be recovered, by means of heat, from a combination of it with *fixed* alcali) I neutraliſed ſeveral ounces of it, condenſed in water, with *volatile* alcaline ſalt, and then added more of the ſame alcali; till the liquor was very ſenſibly alcaline.

Treating this ſolution in the ſame manner as in the 12th and the preceding experiment, I found, on the very firſt application of the heat of boiling water, that the fixed air left the volatile alcali, with which it had been intimately combined, and indeed *ſuper-ſaturated*; and aſcended, or was *diſtilled* over into the inverted phial, in copious ſhowers; perfectly unchanged, from its union with the alcali, and poſſeſſed of its acid, and all its other, qualities.

EXPERIMENT XVII.

Under this title, I ſhall only recapitulate, as the general reſult of all the experiments I have made, with a view to analiſe fixed air, and particularly to detach its acid from it, by means of alcalis;—that this acid is a principle *eſſential* to the conſtituence of this fluid; if indeed it does not conſtitute the whole of it. If a ſmall quantity of alcali be employed, the remaining fixed air, which has been agitated with it, retains as much of its acid quality, as if it had never been ſubjected to the action of the alcali. On the other hand, if a ſufficient quantity of the latter has been agitated with it, in order to neutraliſe the whole of the mephitic *acid*, the factitious *air* diſappears. In ſhort, fixed air, and its acid, if they be not one and the ſame ſubſtance, appear, from all my reſearches into the nature of this fluid, to be, at leaſt, inſeparable companions: they come and go together; ſo that, when the *acid* is deſtroyed, or loſes its diſtinguiſhing characteriſtics, the *air*, at the ſame time, vaniſhes from our notice.

Some of theſe laſt-mentioned experiments have been ſo lately made; and the impreſſion of your ſecond volume is, as you inform me, in ſuch forwardneſs, that I have not time even to hint at the reſults of the numerous collateral objects of inquiry, which the conſideration of this intereſting and fruitful ſubject has ſuggeſted to me; and which I or others may hereafter proſecute. I am pretty confident that I have not been materially deceived in the experiments above related; or been tempted, by a predilection for a preconceived hypotheſis, to draw concluſions not fully warranted by the premiſes. On that ſuppoſition I ſhall terminate this long letter, or rather formal eſſay, which I have not time, however, to ſhorten, with a few miſcellaneous reflections, in the order in which they occur to me.

1. The ultimate deſign of all our experimental reſearches into the properties of natural bodies, is, or ought to be, public utility. On this account, I mention in the firſt place a practical, uſeful, and perhaps important application of the reſults of the eighth, ninth, and tenth experiments. A medical uſe may be made of the proceſſes there deſcribed, in which a *new neutral ſalt* is produced, by combining the mephitic acid with alcaline ſalts; in putrid diſeaſes particularly, and in all thoſe caſes where we would wiſh to introduce a larger quantity of fixed air into the ſyſtem, than can be condenſed by, or combined with, ſimple water. By previouſly diſſolving in this fluid certain quantities of fixed alcaline ſalt, it may be made to receive twice or thrice its bulk, or a ſtill larger proportion, of fixed air. I have not made any experiments purpoſely to aſcertain how far the ſolubility of the *neutral mephitic ſalt* in water extends: but from the 7th experiment it ſhould ſeem to be poſſeſſed of this quality in an unlimited, or at leaſt in a very extenſive degree. Neither have I yet

had opportunities of experiencing the qualities of very ſtrong ſolutions of this new ſaline neutral compound. It is probable that they will be, at leaſt in part, decompounded in the ſtomach, or *primæ viæ*. I, once only, drank eight ounces of alcaliſed water, which had been neutraliſed by about a pint and a half of fixed air; and was very ſenſible of its effects, particularly in my head, for ſome time afterwards. It appeared likewiſe to act pretty ſtrongly as a diuretic.

From the very ſhort experience I have yet had of Dr. Nooth's apparatus (deſcribed in the laſt volume of the Philoſophical Tranſactions) it ſeems to me well adapted to the preparation of this compound, or ſaline Pyrmont water. All the junctures, however, ought to be perfectly air-tight; as it is of advantage that the alcaliſed water ſhould ſtand a few days expoſed to the action of freſh portions of fixed air; that it may be perfectly neutraliſed, and even receive an exceſs of acid.

2. When a moderate quantity of alcaline ſalt has been diſſolved in the water, as, for inſtance, only three or four grains in each ounce, the artificial Pyrmont water, into which I have converted this weaker alcaline ſolution, is of courſe more ſapid, and appears to me more pleaſant, than even that which has been made with ſimple water. It has the ſame acidulous taſte, when the proceſs has been properly conducted; and if it ſhould be deſired to have it ſtill more pungent and acidulous, chemiſtry will furniſh us with various expedients for diſengaging a part of the mephitic acid, at the time the water is drank. This may be effected either by different ſaline compounds, or by naked acids, or aceſcent liquors. For the mephitic acid is let looſe from its alcaline baſis by all the

acids which I have yet had leiſure to try; and even by the ſlight and latent acid contained in wine, or other vinous liquors.

3. Theſe experiments lead us to conſider the common or *mild* alcaline ſalts, as they are called, in a new point of view. They ſhew, that the only true and ſimple alcaline ſalt is the *cauſtic* alcali, either fixed or volatile; which has been reduced to a *pure* alcaline ſtate by the abſtraction of the mephitic acid combined with it, through the ſuperior affinity or attraction of *pure* or *ſimple*, that is, *calcined* calcareous earth. All the others are only *ſubalkaline* ſalts, more or leſs combined with, and in part neutraliſed by, the mephitic acid; and which are capable, as has been ſhewn, of being completely neutraliſed by it.

4. When we expel, and collect, the mephitic acid from an alcali, by means of the vitriolic, or any other acid, the proceſs is perfectly ſimilar to thoſe by which we expel and diſtill other acids from their alcaline, earthy, or metallic baſes, by means of a ſuperior acid. The only material difference is, that the vapours of thoſe acids, though equally elaſtic, are, in general, readily condenſable, and come over reduced into a liquid and palpable ſtate: whereas the vapour of the mephitic acid more obſtinately retains its elaſticity; which it preſerves till a body is preſented to it, to which it has an affinity, and with which it then readily unites. Your *acid air*, that is, the marine acid in a ſtate of vapour, approaches neareſt to it in this reſpect. Like mephitic air, it preſerves its elaſtic or aerial qualities, when confined by glaſs and mercury; and only differs from it in being much more readily and copiouſly condenſed, when water is preſented to it. In that caſe, however, the phlegm which

condenſes *acid air* becomes a ſtrong ſpirit of ſea ſalt; whereas that which condenſes *fixed air*, does it ſo ſparingly, as to conſtitute only a Pyrmont water, or a dilute ſolution of mephitic acid.

5. It is a matter which may be worthy of future inveſtigation, to inquire whether pure *fixed air* be a ſimple or compound ſubſtance; and whether the *mephitic acid* may not be procured, *per ſe*, or in a liquid, viſible, or concentrated ſtate; by the addition of a few *drops* of which, water or other liquors may be impregnated with it to any degree. From the experiments related in a letter of mine inſerted in your former volume, [page 317, 1ſt edit.] as well as from ſome of your own obſervations, it appeared that nitrous air was the *vapour* of the nitrous acid, probably combined with phlogiſton, or ſome other ſubſtance *; to which it owed its elaſticity, or aerial form, and from which it was ſeparable by the admixture of atmoſpherical air. If fixed air be ſimply the vapour of the mephitic acid, the taſk of thus condenſing or concentrating it becomes deſperate. But it may poſſibly be united with ſome *volatiliſing principle*, to which it may owe its elaſticity, and its being ſo ſparingly ſoluble in water. Purſuing this idea, I foreſee many reſources which chemiſtry affords us, for accompliſhing this purpoſe. As I have not, however,

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* The union of this principle with the nitrous acid in nitrous air, is ſo ſtrict, that the latter may be long and forcibly agitated in a phial, not only with water, but even with alcaline ſolutions, or lime-water, without being decompounded. But on holding the phial in an inverted ſituation, ſo as to ſuffer bubbles of atmoſpherical air ſucceſſively to enter, the decompoſition each time viſibly takes place; and the redneſs and effervеſcence appear on every freſh admiſſion, till the whole of the nitrous acid, hereby diſlodged from the other principle, has been combined with the included alcali or earth.

had time to realiſe any of them, I ſhall not enlarge this Eſſay with any of my various ſpeculations on this ſubject.

I am, &c. WM. BEWLY.

See Mr. Bewly's *third Letter*, No. VI.

NUMBER II.

A Letter from Dr. PERCIVAL, *F.R.S. and S.A. to the Rev. Dr.* PRIESTLEY, *on the Solution of Stones of the Urinary and of the Gall Bladder, by impregnating Water with* FIXED AIR.

Mancheſter, June 1, 1775.

Dear Sir,

I flatter myſelf that FIXED AIR is now become an object of the attention of phyſicians; as it has been fully ſhewn that it is capable of being applied to many important medicinal purpoſes. In *pulmonic diſorders*, the *gangrenous ſore throat*, and in *malignant fevers*, the happieſt effects have been experienced from the uſe of it; and I know not a more powerful remedy for *foul ulcers*, as it mitigates pain, promotes a good digeſtion, and corrects the putrid diſpoſition of the fluids. I have related ſeveral caſes, in the Appendix to your treatiſe on air, which evince the truth of theſe obſervations; and ſince the publication of that work, a variety of ſimilar facts have occurred to my learned friend, Dr. Dobſon, in his hoſpital-practice at Liverpool.

But I have a farther and very intereſting diſcovery, concerning the medicinal properties of this ſpecies of factitious air, to communicate to you. About the end of laſt year I was informed that Dr. Saunders, a phya phyſician in London, eminent for his knowledge of chemiſtry, had employed it as a ſolvent of the human CALCULUS.

I was ignorant of the manner in which his trials were conducted, and of the ſucceſs which had attended them: but my curioſity was excited; the acquiſition of ſuch a remedy was flattering to my hopes; and I engaged in the purſuit of it almoſt with as much ardour, as if it had been the philoſopher's ſtone. I recollected that Dr. Black and Mr. Cavendiſh have proved the ſolubility of various earthy bodies in water, either by abſtracting from, or ſuperadding to the fixed air which they contain: and as the human calculus is diſſolved in the former way by lime-water and the cauſtic alkali; it appeared highly probable, that the like effect would be produced on the ſame ſubſtance by the latter mode of operation. Analogy ſeemed favourable to the hypotheſis; and experiment has confirmed it. I have found by repeated trials that *calculi*, extracted from different ſubjects, and varying in ſize, figure, and texture, are ſoluble in water impregnated with fixed air; that this menſtruum is more powerful in its operation even than lime-water; and that though it is inferior in efficacy to the vitriolic acid, and the cauſtic alcali, yet it is more univerſal in its action than either of them. For it is well known (ſee Dr. Dawſon's Experiments, Medical Tranſactions, vol. 2. p. 105) that ſome ſtones, which are diſſolved by the cauſtic alkali, are unchanged by the vitriolic acid, and *vice verſa*; whereas the mephitic water, as far as my obſervations have reached, acts upon every *calculus* which is ſuſpended in it. And I have tried it with thoſe which have ſuffered no diminution of weight from the *menſtrua* above-mentioned. I do not trouble you with a detail of my experiments, becauſe they would exceed the bounds of a letter, and I ſhall probably publiſh them, with ſuch remarks as they may ſuggeſt, on ſome future occaſion.

But I cannot reſtrain myſelf from expreſſing the heart-felt ſatisfaction which I enjoy in the diſcovery of a new lithontriptic medicine, that is at once grateful to the palate, ſtrengthening to the ſtomach, and ſalutary to the whole ſyſtem. Lime-water often nauſeates the patient, deſtroys his appetite, and creates the heart-burn: and the ſoap-ley is ſo cauſtic and acrimonious, that it can be taken only in the ſmalleſt quantity; frequently produces bloody urine; and aggravates the tortures which it is intended to relieve. Both theſe remedies alſo require a very ſtrict regimen of diet, and their qualities are liable to be changed either by acidities, or the fermentation of our food in the firſt paſſages. But the mephitic water may be drunk in the largeſt quantity without ſatiety or inconvenience: it requires no reſtrictions in diet, and its medicinal virtues will be undiminiſhed in the ſtomach or bowels. Perhaps it may be queſtioned whether fixed air can be conveyed by the ordinary courſe of circulation to the kidneys and bladder: In an elaſtic ſtate it certainly cannot; but diſſolved in water, it may paſs through the vaſcular ſyſtem, without creating the leaſt diſturbance or diſorder; and by its diuretic quality, will be powerfully determined to the urinary organs. So ſtrong is the relation that ſubſiſts between mephitic air and water, that they remain firmly combined, although expoſed to conſiderable variations of heat and cold. You found that it required half an hour, even when the boiling heat was

employed,

employed, to expel compleatly the fixed air from a phial of impregnated water; and I have obſerved that it has retained its peculiar flavour ſeveral days, when left in a baſon, with a large ſurface open to the external air.

But to obtain more ſatisfactory evidence upon this ſubject, I filled a bottle with mephitic water, and placed it in a heat of about 98 degrees of Fahrenheit's thermometer. A bent glaſs tube, a quarter of an inch in diameter, properly luted at each end, formed a communication between this bottle and one of lime-water, to the bottom of which it extended. An inteſtine motion ſoon enſued; air-bubbles were ſlowly conveyed into the lime-water; and a white precipitation was gradually formed.

In an hour and a half the lime-water was become turbid; but was quickly rendered quite milky by blowing air into it from the lungs. The mephitic water ſtill retained its briſk acidulous taſte; and when a greater degree of heat (108°) was applied to the bottle which contained it, a briſk inteſtine motion was renewed.

As the vapour of chalk, and oil of vitriol, has been found ſo efficacious in correcting the *ſanies*, and abating the pain of foul ulcers, when externally applied, we may reaſonably preſume that the internal uſe of the ſame remedy will prove beneficial in ſimilar affections of the urinary paſſages. Such complaints frequently occur in practice, and may ariſe either from *calculi* in the kidneys and bladder; from the receſſion of ſcorbutic eruptions, which appeared on the ſurface of the body; from the venereal diſeaſe; from ſtrains; from contuſions; or various other cauſes. And water impregnated with fixed air, ſeems well adapted, by its diuretic, healing, and antiſeptic powers, to waſh off, and ſweeten the acrid matter, to abate the defluxion on the mucous membrane, to contract the flabby edges of the ulcers, and to diſpoſe them to a ſpeedy granulation. If the pain, inflammation, and abſorption of *pus* have excited a hectic fever, the patient may drink plentifully of Seltzer water, which is of a cooling quality, although it abounds with mephitic air: or a ſmall quantity of Rochelle ſalt may be added to the mineral water artificially prepared. Thus will the increaſed action of the heart and arteries, which may ariſe from the ſtimulus of the fixed air, be entirely obviated, without the leaſt diminution of its medicinal powers. And whilſt the ſanction of experience is wanting, reaſon will juſtify the trial of a remedy, which is, at once, ſafe, pleaſant, and efficacious.

In ulcers of the kidneys and bladder, the urine is commonly high coloured, pungent, and of an offenſive ſmell. To aſcertain whether fixed air would correct theſe qualities, I attempted the following diſagreeable experiment.

Repeated ſtreams of fixed air were conveyed into three pints of urine, which had been kept till it was become very putrid, and which emitted a ſtrong volatile odour. I examined the ſmell of it from time to time, whilſt this proceſs was carrying on, and compared it with a portion of the ſame urine which was reſerved as a ſtandard. The pungency of it gradually diminiſhed; it acquired a brighter colour, and was leſs turbid; but its putrid odour ſeemed to be increaſed. Theſe obſervations were made in the evening, and early the next morning I awoke with a violent head-ach, which was attended with a vomiting and a *diarrhœa*. Alarmed at theſe effects, which I attributed to the putrid vapours of the urine, I dropped the proſecution of the experiment; but the ſucceeding day, Mr. Thomas Smith, a young gentleman, who will one day be an ornament to the profeſſion of phyſic, undertook the examination which I had begun: and after attentively comparing together the ſtandard and the urine impregnated with fixed air, he found the latter more offenſively putrid than the former, but without any degree of pungency or volatility. As this experiment was not compleated, I am uncertain whether the urine was ſweetened by the mephitic air. But it is evident that the volatile alkali, generated by putrefaction, was either neutraliſed, diſſipated, or prevented from aſcending by the atmoſphere of fixed air, which filled the upper part of the veſſel. Perhaps this atmoſphere might be the *menſtruum* of the putrid *effluvium*, emitted by the urine, which being then accumulated, would appear to have its fœtor increaſed. In another work, I have related an experiment of Mr. Henry's ſomewhat ſimilar to this, and which ſuggeſted to him the like explanation. A piece of putrid fleſh was ſuſpended twelve hours in a three-pint bottle cloſely corked, and filled with fixed air, which had been ſeparated from chalk by the vitriolic acid. The beef was conſiderably ſweetened, but the air in the bottle was rendered intolerably offenſive.

The waters of Bath, in Somerſetſhire, have been long and juſtly celebrated for their efficacy in the jaundice, and other hepatic diſorders. They abound with fixed air; and it may be of importance to aſcertain whether they derive from this active principle, the power of diſſolving the concretions of the bile, and of removing the obſtructions in the liver. I was induced therefore to try the ſolubility of gall ſtones in mephitic water. But I have yet only a ſolitary experiment on the ſubject to offer to you. A gall ſtone, that had been extracted from a tumour in the region of the liver, was divided into two parts. One of theſe, which weighed fifty-one grains and a half, was immerſed four days in rain water, ſtrongly impregnated with fixed air. The other weighed twenty grains and a quarter, and was macerated in ſimple rain-water during the ſame ſpace of time. The firſt fragment, when carefully dried, was become heavier by one grain, having gained ſo much from the fixed air. In texture and appearance it remained unchanged. The ſecond fragment had loſt one eighth of a grain.

I mean not to draw any deciſive inference from a ſingle experiment. But it is probable that the Bath waters reſolve concretions of the bile, not ſo much by a chemical operation, as by accelerating the ſecretions of the liver, ſtimulating the organs of digeſtion, and invigorating the whole animal ſyſtem. Nature indeed obſerves a peculiar oeconomy in the circulation of the blood through the liver; and as the bile is one of her moſt elaborate fluids, it muſt be difficult to introduce a foreign and unaſſimilated ſubſtance into it. From analogy, however, we may conclude, that this is not impracticable. The milk and the ſaliva are frequently impregnated with adventitious matters; and theſe animal liquors, like the bile, are ſecreted by organs of a particular ſtructure, and for determinate and important purpoſes. A remedy which would paſs unchanged into the ſyſtem of the liver, and medicate the bile, ſo as to render it unapt to coagulate, or enable it to reſolve the concretions already formed, would be a moſt valuable acquiſition * ; and the obſtacles to the attainment of it ſhould rather be regarded as incitements to our induſtry, than apologies for ſupineneſs and deſpair. Such, it muſt be acknowledged, they have proved; as appears from the variety of diſſolvents which have been propoſed and tried. Acids, alkalies, ſoap, ardent and dulcified ſpirits, with freſh vegetable juices, have been recommended. Valiſnerius found that a compoſition of alcohol and oil of turpentine deſtroyed the texture and coheſion of gall ſtones, more perfectly than any other *menſtruum* † ; and Mr. William White of York has fully confirmed this obſervation, by a number of judicious experiments which he has communicated to me. Some time ago I thought favourably of this remedy, and endeavoured to promote the trial of it ‡ ; but farther reflection has convinced me, that the continued uſe of it is more likely to prove injurious than beneficial. Spirituous liquors, of all ſorts, have a peculiarly unfavourable operation on the liver; and it would be abſurd to ſeek a *ſpecific medicine* for the diſeaſes of the bile, in what experience has fatally ſhewn to be a *ſpecific poiſon* to the organ which ſecretes it. Perhaps fixed air, under ſome form or other, may hereafter be found to be the *deſideratum*, which we have been ſo long purſuing. At leaſt, we may be allowed to attribute ſome ſhare of the virtues which the Bath waters poſſeſs, to this ingredient in their compoſition; and when they cannot be employed, to recommend the mephitic water, as an innocent and efficacious ſubſtitute.

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* Vide Medical Tranſactions, vol. 2. p. 165.

† Opere, tom. 3. p. 6.

‡ Eſſays Medical and Experimental, vol. 2. p. 232.

I am, with ſincere reſpect and eſteem,

Dear Sir,

Your moſt faithful and affectionate friend,

THO. PERCIVAL.

P. S. Since this letter was written, the young gentleman, whoſe name I have before mentioned with reſpect, has at my deſire taken large quantities of mephitic water daily, during the ſpace of a fortnight. His urine became impregnated with fixed air, precipitated lime-water, and proved a powerful diſſolvent of the *calculi*, which were immerſed in it.

Dr. Saunders, to whom I have communicated my obſervations on the ſolution of human *calculi* by mephitic water, has lately favoured me with a general account of his diſcoveries on this ſubject. There is a perfect agreement in the reſult of our experiments, and we propoſe to publiſh them together.

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NUMBER III.

A Letter from Dr. Dobson *of* Liverpool *to Dr.* Priestley; *with Caſes of the Efficacy of* Fixed Air *in putrid Diſorders.*

Liverpool, March 29, 1775.

Dear Sir,

My friend Dr. Percival has lately informed me, that you are about to publiſh a ſupplement to your valuable work on fixed air.

The gentlemen of the faculty have not given that attention to the medical uſes of fixed air, which I think it merits: and a late medical writer, (vide Dr. Lettſom's Medical Memoirs of the Gener. Diſpenſ. p. 334.) doubts whether fixed air has any real efficacy even in diſeaſes of a putrid claſs. I have tranſcribed therefore four caſes from a number of others, which are much at your ſervice. If they are too late, or do not coincide with the intention of your preſent publication, pleaſe to return them, that they may be joined with ſome other caſes and practical obſervations, which I ſhall give to the public as ſoon as I have time to tranſcribe them.

That Dr. Prieſtley may long enjoy health, and the world reap the fruits of his philoſophical labours, are the ſincere wiſhes of his

Reſpectful and obedient ſervant,

Matt. Dobson.

Of the Efficacy of FIXED AIR *in Fevers of the putrid Claſs.*

Putrid fevers rarely acquire any great degree of malignancy in Liverpool, or its neighbourhood; and when they do appear, it is generally among the lower ranks of people. A fever of this kind crept into our public hoſpital in the ſpring of the year 1773, and a conſiderable number were infected.

The following hiſtories are tranſcribed from the notes, which were taken during my attendance on the reſpective patients.

Hiſtory I.

Mary Rainford, about 15 years of age, was admitted into the hoſpital on account of convulſions; ſhe was ſubject likewiſe every three or four weeks to vomit large quantities of blood, and was much enfeebled by theſe complaints at the time of her being ſeiſed with the fever.

She firſt complained of pain and weight in the head, pain in the limbs and back, and a great degree of languor and dejection; ſhe had frequent chills, alternating with fluſhes of heat, and got very little reſt. The tartar emetic was twice given, and operated eaſily and powerfully by vomit; a bliſter was applied between the ſhoulders; and a doſe of the following mixture was ordered to be taken every three hours.

℞. Sp. Minderer. ℥viij.
Sacch. Alb. ʒiij.
Sp. Lav. com. ℥ſs. M. Cap. Coch. ij. maj.
tertiâ quaque hora.

She had for common drink lemonade, with ſweet mountain, or barley-water well acidulated; the body was kept ſoluble either by clyſters, or ſome gentle purgative, and the room was well aired by opening the door and windows. But notwithſtanding the ſteady uſe of theſe means, the fever became more and more untoward, and was on the ſixth day accompanied with ſuch dangerous ſymptoms, as made it neceſſary to adopt ſome other method. The eyes were heavy, the conjunctiva red, large petechiæ ſpread over different parts of the body, the tongue was covered with a brown fur, and the teeth with a fur of a blackiſh colour; ſhe was very feeble, got no ſleep, and was frequently delirious, eſpecially in the night. Hitherto the ſtate of the pulſe had been about 120, now it was 135, and very weak.

Fixed air was now directed in the following manner.

℞. Sal. Tartar. ℈j.
Aq. Fontan. ℥ß.
Sacch. Alb. ℈j. M. Sumend. cum Sacc. Limon. ℥ß,
incipiente ebullitione; et omni hora repetend.

From the time ſhe entered upon this plan, the ſymptoms were more favourable, ſhe took no other medicine, and was out of danger in four days.

Hiſtory II.

Alice Rigby was received into the hoſpital for a ſore leg, and during her ſtay was attacked with the fever of the houſe. The progreſs and treatment of the diſeaſe for the firſt week, were nearly the ſame as in the preceding caſe. On the ſeventh day ſhe was extremely weak, got no reſt: there were large petechiæ on many parts of the body, the brain was much affected, pulſe 125, and the tongue little differing from its natural appearance.

Fixed air was now adminiſtered in the ſame manner as to the former patient.—The petechiæ ſoon began to diſappear, ſhe got ſtrength, the pulſe became fuller and ſlower, and the fever was ſubdued in ſix days by the uſe of this medicine alone. The bark was at this time ordered, as an additional ſecurity againſt a relapſe.

Hiſtory III.

March 20. A conſultation was deſired for Ann Knowles, who had been in the hoſpital for a conſiderable time, and was much reduced by a long continued rheumatiſm at the time ſhe was attacked with the fever. This was the ſeventh day of the diſeaſe; and though ſhe had been very judiciouſly treated by the gentleman under whoſe care ſhe had been at firſt admitted, the fever grew daily worſe, and was now accompanied with many dangerous ſymptoms.

I obſerved an extreme languor and dejection; the eyes heavy, the eye-lids half cloſed, and the conjunctiva inflamed. There was a ſtupor, with a muttering kind of delirium, and a continual toſſing and moaning. The pulſe very weak and very frequent, more than 140 in a minute; the tongue moiſt and clear, and not altered from its natural appearance, except that it was of a deep red. The whole body was covered with ſmall petechiæ; ſhe had frequent ſtools, which were extremely offenſive, and her little remains of ſtrength were every hour ſtill more and more exhauſted.

It was agreed, that ſhe ſhould take the fixed air in the ſame way as I had ordered it for the two preceding patients.

March 21. The good effects of this medicine were evident, though the ſymptoms were ſtill urgent and alarming: the ſtools leſs frequent, but offenſive: the pulſe 130, and not ſo languid: in other reſpects the patient was not much altered.

23. The petechiæ diſappearing, the looſeneſs diminiſhed, and the ſtools much leſs offenſive; pulſe 110, ſleeps and gets ſtrength. The medicine was now to be given only every four hours.

24. Stronger and better, pulſe 100, head much clearer, and the tongue has more of its natural red.

26. Pulſe 85; and from this time the fever entirely left her. She took no other medicine, and had no relapſe.

I have directed fixed air both in hoſpital and private practice for a variety of patients, in diſeaſes accompanied with ſymptoms of putrefaction, and with ſucceſs. It would be ſuperfluous to enter into a further detail of particular hiſtories. The following account, however, of the happy effects of fixed air in the ſecond fever of the ſmall pox, is ſo ſtriking a proof of its efficacy in diſeaſes of the putrid claſs, that I muſt tranſcribe it.

Hiſtory IV.

Ann Forbes, ſervant to Mr. Hume of York-ſtreet, Liverpool, had the confluent ſmall pox in Auguſt 1773. The weather was extremely hot, and the ſymptoms ſo very unfavourable, that there did not appear the moſt diſtant hope of her recovery. Particular care was taken to have a conſtant ſupply of freſh air, and the antiphlogiſtic treatment was ſtrictly purſued during the inflammatory fever. The diſeaſe was now advancing into the putrid ſtage, and the ſecond fever commenced with little or no appearance of ſuppuration.

Notwithſtanding every precaution with reſpect to the free acceſs of air, change of linnen, and every circumſtance of cleanlineſs, the unlimited uſe of oranges, lemonade, and wine negus, this poor creature was the moſt miſerable object I ever beheld. She became extremely offenſive, and had the appearance of one continued maſs of putrid ichor: the pulſe ſmall and rapid, and ſhe had a conſtant reſtleſſneſs with inexpreſſible anxiety.

A purgative was directed, and a glaſs of ſweet mountain after every ſtool. The ſymptoms, however, became more and more alarming, the offenſiveneſs was almoſt intolerable, and ſhe was frequently ſick, agitated, trembling, and like one about to expire. In this urgent ſituation, I determined to try the effects of fixed air, and it was given in the manner already mentioned.

The nurſe and attendants ſoon obſerved an agreeable change. In 24 hours, the putrid ſtench was much diminiſhed, the breath of the patient was not near ſo offenſive, and the room was very tolerable compared with what it had hitherto been. She was cooler, had leſs anxiety, and the pulſe became fuller and ſlower. In two days more, ſhe was ſtill much better: and by repeating the purgative, giving wine occaſionally, and perſevering in the uſe of the fixed air, her recovery was ſurprizingly ſpeedy and perfect.

NUMBER IV.

Extract of a Letter from JOHN WARREN, *M.D. of* Taunton, *to Dr.* PRIESTLEY, *with a medical Caſe, proving the Uſe of Clyſters of* FIXED AIR *in a putrid Diſeaſe.*

Taunton, Oct. 3, 1775.

Sir,

In compliance with your requeſt, I have done myſelf the honour of ſending you an account of a medical caſe, in which the application of *fixed air* proved ſo remarkably ſucceſsful, as to leave no room to doubt of the great advantages the medical world might derive from its uſe, in putrid diſeaſes of the worſt and moſt formidable ſpecies.

I have for many years paſt been ſtrongly prepoſſeſſed in favour of the utility of fixed air in certain medical caſes, from its peculiar virtue verified by Dr. Macbride's experiments, in ſweetening putrid fleſh, and reſtoring to it that texture, which it muſt of courſe have loſt by undergoing ſuch a change; nor have I, in the practice of my profeſſion, found myſelf diſappointed of the advantages which I flattered myſelf from theory, might be derived from its uſe.

I have latterly employed it in almoſt every putrid caſe that offered; and though I am by no means ſo partial to its virtues, as to attribute the whole merit of a recovery to it alone, when probably other medicines, with which it was joined, were alſo entitled to their ſhare, yet I am thoroughly convinced, that the ſucceſs I have met with, in the treatment of putrid diſorders, is, in a great meaſure, to be aſcribed to the large quantities of this fluid which I have conſtantly enjoined my patients to make uſe of.

Various have been the modes which I have adopted for introducing it into the ſyſtem—I order it frequently to be given by clyſter, ſometimes to be inhaled by the mouth (particularly in ulcerated ſore-throats with the greateſt advantage) and it is with the ſame view alſo of correcting putrefaction, that the common drinks, which I allow my patients, are almoſt all of them impregnated with this ſpecies of air, as Pyrmont water, briſk ſmall beer, currant wine, and the like.

I would beg leave here to ſubjoin a practice I have for ſome time paſt found productive of the moſt beneficial conſequences in the treatment of putrid diſeaſes in children—It is commonly known, that phyſic of every kind is to them peculiarly obnoxious, and thouſands annually fall a ſacrifice to diſorders from a mere antipathy to it. Therefore, whenever I perceive a child utterly averſe to take medicine, if the

Peruvian bark is proper, I give it by clyſter, and order the milk (its beſt and moſt common vehicle when given in this manner) to be as thoroughly impregnated with fixed air as poſſible.

I have the honour to ſubſcribe myſelf,

Sir,

Your very obedient ſervant,

JOHN WARREN.

The Medical Caſe.

Mr. C———, aged 23, a gentleman of great temperance, and of a good conſtitution, laboured under an irregular nervous fever for the ſpace of ten weeks, attended for the moſt part with delirium, and almoſt conſtant watchfulneſs.

At the expiration of this period, ſymptoms of putridity began to make their appearance, ſeemingly however more owing to emaciation, and to the long continuance of his diſeaſe, than to any original contagion. The Peruvian bark had from time to time, particularly during the latter ſtages of his illneſs, been adminiſtered to him, which, on the appearance of putrefactive ſymptoms, was increaſed to the quantity of two ſcruples, given in the form of a bolus, with thirty drops of elixir of vitriol, every two hours. Every thing he drank was acidulated with the juice of lemons or oranges, and his common liquor was Port wine mixed with Pyrmont water. This courſe was perſiſted in for ſome days, the putrid complaints however increaſed, and ſo great was the fœtor emitted with his breath, and ariſing from his body, that notwithſtanding his being ſupplied with a conſtant ſucceſſion of freſh air, and though large quantities of vinegar, lavender-water, and rue, with other odoriferous ſubſtances, were conſtantly made uſe of to purify his room, yet all were found perfectly ineffectual. His ſtools, which at this period conſiſted of little elſe than putrid blood, and which came from him in great abundance (in the whole to the amount of many pounds) were abſolutely intolerable, nor was it without much difficulty that the nurſes could be induced to remain any longer near him.

Univerſal languors, with almoſt total inſenſibility, now ſupervened: an earthy coldneſs diffuſed itſelf through every part of his body, nor were the hotteſt fomentations, though continued three hours together, capable of procuring him any degree of warmth.—Every breath he drew ſeemed to be his laſt.—In this deplorable ſituation he continued a whole day, his boluſes were omitted through neceſſity, and with difficulty we could, from time to time, get him to ſwallow a few ſpoonfuls of ſome warm cordial medicine; which, however, by continually repeating, began at length, in ſome degree, a little to revive him.

I now ordered him clyſters of *fixed air*, of which a large bladder full, containing near two quarts of air, was every three or four hours injected, and his bark boluſes were again given to him, as often as his ſtomach would allow him to take them. In the ſpace of eighteen hours, the cadaverous fœtor ariſing from him, began to abate; large vibices, or putrid blotches, were now, for the firſt time, diſcovered on almoſt every part of his body, his pulſe however was better, and his warmth in ſome little degree returned; the boluſes and clyſters were ordered to be continued. In four or five days the noiſome ſmell became imperceptible, the vibices gradually diminiſhed, and his fever left him. He is now perfectly recovered, and a living miracle of what fixed air, under Divine Providence, is capable of effecting on the human oeconomy, in caſes of the worſt and moſt putrefactive nature.

JOHN WARREN.

NUMBER V.

A Letter from Mr. MAGELLAN *to Dr.* PRIESTLEY, *on the Subject of* DEPHLOGISTICATED AIR.

Dear Sir,

Among the many important diſcoveries for which the philoſophical world is indebted to you, chiefly in that new and extenſive branch of natural knowledge reſpecting *different kinds of air*, a very ſtriking one is the exhibiting, in the form of this fluid, many ſolid bodies, and moſt of the known acids. It is with the moſt pleaſing aſtoniſhment that I have always beheld that experiment, by which any unprejudiced mind muſt be convinced, that atmoſpherical air, even the pureſt, and the fitteſt for animal reſpiration, is produced by heat from a mixture of nitrous acid with any dephlogiſticated earth, as *red lead*, *chalk*, &c. For, after having extracted from this mixture all the air that fire can expel, which is a prodigious quantity, when you repeatedly add freſh nitrous-acid to the reſiduum, you get a freſh quantity of this pureſt air, till all the earthy ſubſtance diſappears.

This, however, being a very tedious proceſs, when carried on with the moſt ſcrupulous attention, it came to my mind, that it would be ſufficient to examine at the end of the firſt proceſs, whether the reſiduum from the above mixture contained any part of the nitrous acid which had been put to it. To clear up this doubt, I kept for that purpoſe, with your approbation, the very ſame reſiduum of that proceſs of the kind which we made a few days ago, to ſhew this wonderful kind of air to his Highneſs Prince Orloff, and with it I made the following experiments.

I put into a large phial a weak blue tincture of archil, and after mixing it well, I poured two thirds of it into two ſmaller phials, in one of which I put a good quantity of the ſaid reſiduum, and into the other as much of the dried mixture of red lead, with nitrous acid. The blue colour of this laſt phial diſappeared in a few ſeconds, leaving the liquor almoſt limpid and tranſparent; but the other tincture, with the reſiduum out of which the air had been expelled, ſhewed no change of colour, when compared with the remaining quantity of the tincture left in the large veſſel.

I repeated this morning the ſame experiment with the tincture of turneſole, and found the ſame effect, with this only difference, that the tincture turned reddiſh in the glaſs, which contained the dried mixture of red lead with nitrous acid, whilſt the other kept its blue colour.

This ſeems to evince, with the greateſt certainty, that the nitrous acid in this experiment is entirely ſet free by the action of the fire, in the form of air, and being at the ſame time combined with ſome part of the earthy matter, becomes reſpirable air. It is remarkable that this air has no acid in it, as may be concluded from the effect of ſhaking it with the above tincture in a phial, which I did: for it does not change the blue colour; whereas if the ſame operation is made with fixed air, it is changed almoſt inſtantaneouſly into a very decided reddiſh colour, as is well known.

Now ſince the air produced from the mixture of earth with nitrous acid, not only does not diſcover the leaſt acidity, but proves to be the pureſt and the moſt wholſome for animal reſpiration, it plainly demonſtrates that either air is not an *element*, or acid is not one, as ſome chuſe to aſſert: ſince nitrous acid is reduced into air, together with the earth, in the above experiment, without leaving behind any acidity to impart it to the air which comes out from it. As to myſelf, I ſhould rather think there is a tranſmutation of *elements* into one another, if ſuch *name* may be uſed in this caſe; for we ſee by the above experiment, that acid and earth are tranſmuted into air, and by the experiments of Mr. *Godfrey*, publiſhed in 1747, it ſeems that *water* is convertible into earth.

If you think the above may be any elucidation or confirmation of your experiments on this ſubject, you are at liberty to make what uſe of them you pleaſe.

Dear Sir, Yours, &c.

London, 20th Nov. 1775.

J. H. DE MAGELLAN.

NUMBER VI.

Mr. BEWLY's *third Letter to Dr.* PRIESTLEY, *containing farther Experiments and Obſervations on the mephitic Acid. See p.* 337, *&c.*

Sir,

On peruſing ſome of the firſt ſheets of your new volume, which you have been ſo obliging as to tranſmit to me, together with a few pages of a treatiſe juſt publiſhed by Sig. Landriani of Milan *, relative to the ſubject of my former letters; I find that ſome of the foreign philoſophers, who acknowledge the exiſtence of an acid in fixed air, conſider it only as an extrinſical principle furniſhed by the particular acid that has been uſed in the proceſs for procuring it. Sig. Landriani, in particular, who, from the ſpecimen which you have ſent me of his work, appears to be a very intelligent and accurate inquirer, there affirms, that the fixed air expelled from chalk by the *vitriolic* acid, and received into an inverted phial plunged in mercury, produces, on the admixture of alcaline air, cryſtals of *vitriolic* ammoniac; and that when the *nitrous* acid has been employed, a *nitrous* ammoniac is formed, which deflagrates without the addition of any phlogiſtic matter.

The ſame philoſopher affirms (with a view to ſhew that the acid in fixed air is only a modification of the particular acid employed in generating it) that a given quantity of the infuſion of turneſole, which will be changed red by a *certain quantity* of fixed air expelled from chalk by the ſtrong vitriolic acid, will not have it's colour altered by an *equal quantity* of fixed air procured by a weak vegetable acid, as that of lemons. He further aſſerts, that fixed air betrays, even by it's ſmell, and by the flavour which it imparts to the water ſaturated with it, the particular acid to which it owes it's acidity. He particulariſes the fixed air expelled from chalk by the nitrous acid, and that expelled by the juice of lemons; obſerving, that the particular ſmells and flavours of theſe two acids may be diſtinctly perceived in the fixed air reſpectively procured by them, as well as in the water impregnated with them.

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* Intitled, *Ricerche Fiſiche intorno alla ſalubrità dell' Aria.*

In theſe particulars I apprehend that Sig. Landriani has been deceived by appearances, and particularly by attributing to fixed air, in general, the attributes of fixed air accidentally *ſophiſticated* by foreign admixtures. This fluid undoubtedly, like all other fluids, is liable to receive taints or impregnations from any ſubſtances capable of being elevated into vapour, and of being diſſolved or ſuſpended in it. The hypotheſis maintained by him and by other philoſophers on the continent, with reſpect to the foreign or adventitious origin of the acid in fixed air, very naturally occurred to myſelf at the beginning of this inquiry: but the experiments and obſervations contained in my two former letters, as well as others which I ſuppreſſed, obliged me to renounce it, and to conſider fixed air as an *original* acid, which does not owe it's acidity, much leſs it's exiſtence, to any of the acids, or other *media*, which are employed in generating it. I could not indeed entertain any doubt of the truth of this laſt opinion, when I had procured fixed air (as is related in my firſt letter) exhibiting unequivocal marks of acidity, (that is, reddening the infuſion of litmus, or neutraliſing alcalis) when expelled from chalk, the pureſt magneſia, and volatile alcaline ſalts, *by heat alone.* It may not be amiſs however, before I proceed further on this ſubject, to take this opportunity of adding ſome of my former Obſervations relating to it, which I before omitted to mention; particularly thoſe reſpecting the nature of the acid in the fixed air procured from chalk, by the *vitriolic acid*, and which Sig. Landriani calls *vitriolic fixed air.* That this acid is not vitriolic acid, under any of it's modifications known to us, appeared to me to be evident from the following conſiderations.

1. The acid in fixed air, thus obtained, diſſolves a mild calcareous earth †, and on evaporating the water by means of heat, or adding an alcali, a mild calcareous earth is precipitated; whereas the vitriolic acid will ſcarce diſſolve a ſenſible portion of the ſame earth, and the precipitate is a *ſelenite.*

The common or fixed, as well as the *volatile* or *ſulphureous*, vitriolic acids, when neutraliſed with vegetable alcali, form neutral ſalts which continue neutral, though expoſed to the air and to heat; the firſt conſtituting vitriolated tartar, and the ſecond, the *ſal ſulphureus* of Stahl; which laſt, on expoſure to the air, loſes only the *phlogiſton* to which it owed its volatility, and conſtantly retains its neutral quality: whereas the neutral *mephitic ſolution*, or the combination of vegetable alcali with the acid of *(vitriolic)* fixed air (in the 14th Experiment) parts with that acid in the common temperature of the atmoſphere; and on the total evaporation of the water, the fixed alcali is left, very little changed by the experiment.

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† See Letter 2d, Experiment XV.

3. Solutions of vitriolic ammoniac will bear being evaporated over the fire to a pellicle; and, on cooling, perfectly neutral cryſtals are formed; but the ammoniacal ſolution formed by the union of fixed air (obtained as above) with volatile alcali, could not be made to furniſh cryſtals; as the greateſt part both of the acid and the alcali flies off, though expoſed only to the common heat of the atmoſphere.

4. The preſence of the ſmalleſt portion of the vitriolic acid, combined with alcaline ſalts or earths, is eaſily detected by the precipitation of a *turpeth mineral*, on the addition of a ſaturated ſolution of mercury in the nitrous acid; whereas a ſtrong *neutral mephitic ſolution*, treated in the ſame manner, furniſhes only a white precipitate.

I ſhall now add ſome other obſervations of a ſimilar kind; as it may be alledged, though no proofs have been offered for that purpoſe, that the vitriolic acid may poſſibly be volatiliſed, and acquire ſome new qualities, or may have its uſual properties diſguiſed, in conſequence of combining it with calcareous earths or alcaline ſalts, in the common proceſs for obtaining fixed air. I ſhall therefore proceed to conſider the peculiar qualities aſcribed by Sig. Landriani to the acid contained in the fixed air which has been procured from theſe ſubſtances by means of the *nitrous acid*.

I thought it very probable, when I entered on this inveſtigation (as I have already hinted in one of my former letters, that, conſidering the very volatile nature of the nitrous acid, ſome part of it might be elevated, during its effervеſcence with the chalk, either in the ſtate of a ſimple vapour, or in the more compound form of *nitrous air*, ſo as to mix with and contaminate the fixed air obtained by its means. For ſimilar reaſons, reſpecting the marine acid, I avoided, in the preceding analyſis, employing any fixed air, procured by means of either of theſe two acids. I made however the following experiments on the preſent occaſion.

EXPERIMENT XVIII.

Having more than once prepared *artificial Pyrmont water* with fixed air, obtained by *ſpirit of nitre* and chalk, I could never diſtinguiſh it, either by its taſte or ſmell, or *ſtrength*, from the artificial Pyrmont water procured by means of the juice of lemons, or even the weak vegetable acid in cream of tartar, combined with ſalt of tartar. On neutraliſing the firſt mentioned waters with ſalt of tartar, and ſuffering a part of the phlegm to evaporate, I ſometimes found that paper, dipped into the liquor, then dried, and applied to a hot cinder, exhibited, as indeed I expected, ſome very ſlight ſymptoms of the preſence of the nitrous acid, by a faint deflagration. On a further evaporation, the liquor, which was originally neutral, or even ſubacid, had acquired a taſte manifeſtly alcaline: a conſiderable part of the acid which had before neutraliſed it (that is, according to my theory, the *mephitic* acid) having flown off; while the nitrous or foreign vapours, which had *accidentally* been ſuſpended in the fixed air, and which had been condenſed by the water, combined with and fixed by the alcali, appeared to have produced theſe faint appearances of deflagration.

EXPERIMENT XIX.

Though I failed of procuring cryſtals from a neutral, but weak mephitic ſolution, when it was expoſed to the atmoſphere *; I ſucceeded, on uſing the ſtrongeſt alcaline lixivium, and carrying on the proceſs in the *medium* of fixed air.—A large quantity of fixed air, procured by the *nitrous* acid, having been thrown up into a ſmall quantity of the ſtrongeſt *lixivium tartari* confined by quickſilver, ſmall cryſtals were almoſt inſtantly formed on the ſides of the glaſs; and the liquor, after a repetition of the proceſs, had condenſed 12 or 14 times its bulk of fixed air: *but theſe cryſtals were not found to be nitre*; nor did they, or the liquor, exhibit any ſtronger marks of the preſence of the nitrous acid, than thoſe mentioned in the preceding experiment. On the contrary, the cryſtals, in particular, being carefully collected and thrown on a red-hot coal, did not detonate, but ſome of the particles bounced, or flew about; in conſequence, doubtleſs, of the impriſoned fixed air being ſuddenly let looſe by the great heat; and they were finally converted into fixed alcali. Volatile alcaline ſpirit, in the cauſtic ſtate, treated in the ſame manner, preſented nearly the ſame phænomena, *mutatis mutandis*. In ſhort, the fixed air, in both caſes, whenever it exhibited any appearances of the preſence of the nitrous acid, ſeemed only to have been adulterated with the vapour or fumes of that acid, ſuſpended in it, and condenſed along with it.

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* See Experiment XIV.

The objection which, as I have above hinted, might be made with reſpect to the *vitriolic* acid, does not ſeem to be in any degree applicable to the preſent caſe. Sig. Landriani cannot allege, conſiſtently with his own experiments, and his deductions from them, that the *nitrous* acid is ſo very materially changed in the act of efferveſcence with calcareous ſubſtances, as to have its nature ſo far altered, ſo that on being afterwards neutraliſed with fixed or volatile alcalies, it ſhould be almoſt totally deprived of its *detonating* quality :—as the only proof which he offers of its preſence in the fixed air expelled by it—(the *detonation* of his nitrous ammoniac) is founded on its ſtill retaining this very property. The reader, however, has ſeen how very faintly it exhibited this criterion of its preſence, in the two preceding experiments.

With reſpect to Sig. Landriani's experiments here referred to, and in which he mixed his ſuppoſed different ſpecies of fixed air with alcaline air, in the *dry way*, or in phials ſtanding in mercury; I ſhall only obſerve that I do not readily conceive how, by a proceſs of this kind, he procured a *ſufficient quantity* of *nitrous* ammoniac, ſo as to aſcertain its nature, by detonation; or of *vitriolic* ammoniac, ſo as to *analiſe* it, and know it to be ſuch. All that he ſays relative to the laſt-mentioned proceſs, in the pages now before me, I have given below *. I have more than once indeed *ſeen* theſe different *airs* condenſed on the ſides of a phial, but have been contented with amuſing myſelf by viewing the various configurations of the cryſtals with a ſmall magnifier, as I deſpaired of being able to collect them in a ſufficient quantity, to ſubject them to a chemical analyſis. Nor have I thought it neceſſary to proſecute this particular mode of enquiring into the true nature of the acid in fixed air, even ſince I read theſe pages of Sig. Landriani, and thoſe ſheets of the preſent work, from which I find that Sig. Fontana, and other philoſophers, maintain a doctrine contrary to that which has been advanced in theſe papers. On ſome of my former experiments, and on one of them in particular, I think I may ſafely reſt the merits of my preſent hypotheſis.

Your readers will recollect that, in my Vth Experiment, a pure and *acid* fixed air was expelled, *merely by means of heat*, from pure Magneſia, contained in a *glaſs* veſſel *hermetically* connected with a bent tube. As that Experiment, however, was left ſomewhat imperfect, by the accidental rupture of my apparatus, which I could not repair or renew; I thought it of ſufficient importance to require a careful repetition on the preſent occaſion. I have accordingly more than once repeated it with the utmoſt attention; and as the ſimple apparatus which I employed for this purpoſe may eaſily be procured, and the proceſs be as eaſily repeated by any one, I ſhall minutely deſcribe the apparatus, and relate all the material circumſtances attending the experiment.

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* After obſerving that long and ſlender cryſtals were formed on the ſides of the phial in which the alcaline and *vitriolic fixed air* were mixed, he only adds, 'Ciò fatto raccolganſi queſti criſtalli, e ſe ſottopongano all' analiſi, che ſi troveranno eſſere un vero ſale ammoniaco vitriolico, a cui tante maravaglioſe virtù attribuiſce il chimico Glauber.' Ricerche Fiſiche, &c. p. 48.

EXPERIMENT XX.

I took a very long, ſmall, and thick green glaſs phial, ſuch as Hungary-water is ſometimes ſold in, and adapted to it a perforated cork, through which paſſed a bent tube of a ſmall bore. I filled this phial with the pureſt Magneſia, preſſing it down, that it might harbour as little common air as poſſible, in the interſtices between its particles. Having ſecured the cork with ſtiff cement, I put the phial into a crucible, where it was ſurrounded with ſand to the height only of 2 or 3 inches, and which was placed in a ſmall chafing-diſh containing lighted charcoal. In conſequence of the tallneſs of the phial, the apparatus remained air-tight, to the end of the proceſs; as the cork and the cement were not affected by the heat, even when the lower part of the phial was red-hot.

Suffering the air to eſcape while the ſand, crucible, &c. were heating, I did not begin to collect any till I judged that the Magneſia was pretty hot. I continued the proceſs till it ceaſed to emit air. For various purpoſes I collected the produce in different phials containing water or other liquors. The reſults were as follow:

1. Though that part of the Magneſia, which was ſubjected to a moderate red heat, did not, as I afterwards calculated, originally weigh above 1 drachm

and a half; I eſtimated that it furniſhed above 30 ounces of fixed air.

2. *All* the fixed air contained in the Magneſia which occupied the loweſt part of the phial, was found to have been expelled from it: at leaſt, on putting ſome of it afterwards into water, and adding ſpirit of vitriol to it, it was diſſolved in the acid liquor without the leaſt appearance of effervеſcence.

3. *The whole of this fixed air* (except the firſt 3 or 4 ounces which were neceſſarily mixed with ſome common air) *poſſeſſed all the qualities of the pureſt fixed air procured by means of the ſtrongeſt acid ſpirits.* No difference could be perceived in its properties; except that, probably for the reaſon juſt mentioned, it ſeemed to come over more and more pure as the proceſs went on; *and the very laſt ounce expelled from it* (and which appears, from (2) to have been the very laſt ounce that it contained) *was as* acid, *and was as readily and fully abſorbed by water, as any of thoſe that preceded it.*—To be more particular:

4. During different periods of the proceſs, certain portions of this fixed air neutraliſed as great a quantity of ſalt of tartar, as could be neutraliſed by equal quantities of the pureſt fixed air obtained by means of the ſtrongeſt acid ſpirits.

5. Towards the latter end of the proceſs, two eight-ounce phials filled with rain-water were ſucceſſively agitated with freſh portions of this fixed air, till they were nearly ſaturated. Filling up the ſpaces occupied by the *reſidua* with water, and throwing up more fixed air, I placed them, inverted, in a baſon of mercury. After ſtanding a night, during which they each abſorbed an additional ounce or two of fixed air,

their

their contents were poured out.—I do not remember having ever taſted any artificial Pyrmont-water, made with the vitriolic acid, more briſk and acidulous than this, produced by plain Magneſia, without the intervention of any acid.

As I cannot avoid laying particular ſtreſs on the reſults of this ſimple calcination of Magneſia; I think it worth while to trace, from it's origin, the fixed air contained in this ſubſtance; by explaining the *rationale* of the proceſs by which Magneſia is procured, according to the principles aſſumed in theſe papers.

The *Epſom Salt*, from which Magneſia is uſually procured, is a neutral compound, formed of the earth of Magneſia combined with the *vitriolic acid*. From this ſubſtance the Magneſia is precipitated, by adding to it a ſolution of ſalt of tartar; that is, (according to the preceding theory) of an alcaline ſalt combined with the *mephitic acid*. On mixing together the ſolutions of theſe two compounds, the two different acids change baſes. The *vitriolic* acid deſerts the magneſia, to unite with the alcali, with which it forms a vitriolated tartar; at the ſame time expelling from the alcali the weaker *mephitic* acid, which inſtantly occupies its place, by uniting with the magneſia, now deſerted by the vitriolic acid.—So that, in fact, all the fixed air, or mephitic *acid*, which, in the preceding experiment, was expelled from the magneſia by fire, had originally reſided in the *ſalt of tartar* employed in the preparation of the magneſia: but as fixed air cannot be expelled from this ſalt by *heat*, it was therefore, in this Experiment, transferred from it to another body (the earth of magneſia) from which it could be *thus* expelled with eaſe. The Experiperiment, in fact, is as ſatisfactory as if the fixed air had been immediately and directly expelled from the alcaline ſalt itſelf by fire.

It may be proper to obſerve that, in the preparation of pure magneſia, the vitriolated tartar abovementioned is carefully waſhed away by repeated ablutions with hot water. But granting, for argument's ſake, that the magneſia were not perfectly free from this vitriolic ſalt, or that ſome portion of vitriolic acid, ſuppoſed ſtill to adhere to it, were capable of being volatiliſed by the fire, and of being ſuſpended in the fixed air;—neither of which ſuppoſitions are, however, admiſſible;—it is impoſſible from hence to account for the *large quantity* of acid procured from the magneſia in the preceding experiment, when the reader recollects that 30 ounces of *acid mephitic vapour* were expelled from it, and calculates, from the rough eſtimate given in my 11th Experiment, the quantity of ſalt of tartar that it would neutraliſe. It would indeed be abſurd to ſuppoſe that this large quantity of acid could be furniſhed either by any vitriolated tartar, or by any ſuperfluous and diſengaged vitriolic acid, ſtill adhering to the magneſia. The ſum of the matter is, that acid air reſides in mild alcalis, from which it cannot be *directly* expelled, but by other and ſtronger acids; to which acids, however, it does not owe its acidity, as Signiors Landriani and Fontana affirm, though it may accidentally be adulterated with them.

This experiment appeared to me ſo deciſive of the queſtion in diſpute (to ſay nothing of ſome others contained in my former letters, particularly thoſe in which *acid* fixed air is expelled, *by heat*, from volatile alcalis)

alcalis) that I could not think it neceſſary to repeat many ſimilar proceſſes with chalk, or other calcareous earths. It happens fortunately that *all* the fixed air in magneſia can be expelled from it by fire, even in *cloſe* veſſels (at leaſt not having a free communication with the atmoſphere) and with a moderate degree of heat; whereas I have frequently found, as you have likewiſe obſerved, p. 119, that chalk will not *generally*, (for there is a great difference in different ſpecimens of this ſubſtance) part with much of its fixed air, under ſimilar, or even more favourable circumſtances. And further, the greateſt part of the fixed air thus expelled from it, is frequently combined with phlogiſtic or other matters, which diſguiſe it, and render only a ſmall part of it ſoluble in water.

I find nevertheleſs, from your preſent work, that the foreign philoſophers lay much ſtreſs on the circumſtance, '*that the air expelled from chalk, in cloſe veſſels, will not render water acidulous*.' I have always however found, that a ſufficient quantity might be expelled from it to redden the infuſion of litmus, and ſometimes to give a ſenſibly acidulous impregnation to a ſmall quantity of water. The fact is, that calcareous earths cannot be *calcined*, as magneſia may, in cloſe veſſels. Since I peruſed your ſheets, and the above-mentioned pages of Sig. Landriani's treatiſe, I made the following Experiment relative to this object.

EXPERIMENT XXI.

After having expelled a few ounces of air from 2 or 3 drachms of chalk, expoſed to a moderate red heat, in a glaſs *veſſel*, as in the preceding Experiment; and having kept the phial in this ſand-heat till it would furniſh no more, I found that a part only of this air poſſeſſed the properties peculiar to fixed air. But I ſoon diſcovered the cauſe of theſe appearances. On examining the chalk after it was cold, I not only perceived, as you too have obſerved, that it ſtill effervеſced moſt violently with acids, but, which is a much more deciſive circumſtance, I found that *nearly the whole of its fixed air ſtill remained in it*; for on throwing the chalk into two ounces of water, I obſerved that it had not acquired the property of communicating, even to this ſmall portion of water, the taſte of *lime-water*; nor, after ſtanding a day or two, was any perceptible cruſt formed upon its ſurface. And yet, from Dr. Black's well-known experiments, it is clear, that if even a *ſingle grain* of this chalk had been calcined, or deprived of *all* its fixed air, it muſt have converted the 2 ounces of water into a pretty ſtrong lime-water. In ſhort, it evidently appeared, that the mephitic acid which, when expelled, ſhould render the water acidulous, ſtill remained in the chalk at the end of the experiment; and that the greater part of the air that did come over, was either not fixed air, or was fixed air enveloped in phlogiſtic matters, or otherwiſe ſo altered, as not to exhibit its uſual properties; whereas *acids* expel fixed air from calcareous ſubſtances wholly, and in a ſtate of purity, equal to that in which it is expelled, by *fire*, from magneſia; though even in this laſt caſe, it ſuffers a remarkable change, when the magneſia is calcined in a gun-barrel.

Before I conclude this enquiry, I ſhall take notice of a curious and difficult problem in chemiſtry, relative to fixed air, and which no one, I believe, has yet attempted to reſolve. In the decompoſition both of *common* and *cubic* nitre, by deflagration with charcoal in a red-hot crucible, it is very remarkable that the alcaline baſis of the nitre is in both caſes left, not in a *cauſtic* ſtate, as might be expected, but replete with fixed air, or in a *mild* ſtate. It ſeems, at firſt ſight, pretty evident that the alcaline ſalt acquires this large quantity of fixed air at the inſtant of the deflagration †; and as there are only two ſubſtances preſent from which it can acquire it, the nitrous acid and the charcoal, it may be conjectured — (and indeed ſome of your experiments contained in this volume ſeem to favour the idea) — that ſome part of the nitrous acid, which, as to ſenſe, aapears to be totally diſſipated in the proceſs, may aſſume the modification of fixed air, and be inſtantly condenſed and combined with the alcali under that form.

This ſolution, admitting it to be juſt, overturns the hypotheſis of Sig. Landriani, and the foreign philoſophers above mentioned: for, ſuppoſing fixed air to be afterwards procured from this alcali, by means of oil of vitriol, marine acid, the acid of lemons, or, in ſhort, any other acid than the *nitrous*; their theory would oblige them to aſcribe it's acidity to the particular adventitious acid employed in the expulſion of it: whereas, according to this ſolution, the acid in the fixed air muſt, in all theſe caſes, be the nitrous. I muſt not however omit to obſerve that it appears, from one of the ſheets of the preſent volume now before me, (page 214.) that charcoal centains a conſiderable quantity of fixed air; from which poſſibly the alcali, deſerted by the nitrous acid during the deflagration, may obtain that principle.—The queſtion certainly deſerves to be further inquired into; as a juſt ſolution of it promiſes to throw conſiderable light on the intimate nature, or *geneſis* of fixed air.

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† Unleſs it ſhould be ſuppoſed that it attracts it afterwards from the atmoſphere, during the ſubſequent part of the proceſs; when it is generally kept in a red heat for half an hour after the deflagration is over: — a aircumſtance which I have not enquired into, but whieh might be aſcertained by examining it immediately after the deflagration.

On the whole, I think it will appear evident from the preceding Experiments, and particularly the XXth, that the acid contained in fixed air, procured in the uſual method, by means of oil of virriol, and other acids, is not, as Sig. Landriani and others aſſert, merely the attenuated and diſſolved vapour of the foreign acid employed in the proceſs; but that it is a diſtinct principle, expelled, in an acid ſtate, from the body to which theſe ſtronger acids had been applied. The mephitic acid has, in all my Experiments, appeared (impurities excepted) to be an invariable, homogeneous ſubſtance, which does not exhibit any of thoſe varieties which might certainly be expected in it, if it owed its exiſtence to acids differing ſo very conſiderably from each other in their properties, as thoſe uſually employed in the procuring it. It is not my preſent deſign to deduce the generation of the mephitic acid, *ab ovo uſque*—. It is ſufficient to obſerve that it appears from its qualities to be as diſtinct from the vitriolic, nitrous, and other known acids, as they are from each other. They may all, as Becher and Stahl long ago ſuppoſed, be only modificadifications of one and the ſame primitive and univerſal acid. But this leads to an enquiry utterly foreign the object I propoſed to myſelf in proſecuting theſe experiments.

I am, &c.

Great Maſſingham,
Nov. 27, 1775.

W. BEWLY,

P. S. I willingly lay hold of this opportunity of following your example, in rectifying a miſtake of Sig. Landriani's reſpecting myſelf, which occurs at page 23 of his treatiſe above referred to; into which he has probably been led, either through his imperfect knowledge of our language, or the miſtake of a tranſlator. He there repreſents me as maintaining 'that nitrous air is nothing more than *common air*, containing the nitrous acid diſſolved in it, combined with phlogiſton.'—On the contrary, I concluded, from my experiments related in your former volume, page 317, that nitrous air conſiſted of the nitrous acid combined with phlogiſton; and ſo far from conſidering common air as a *component* principle, or the *baſis*, of nitrous air, I ſhewed that an addition of common air was neceſſary, in order to *decompound* and condenſe it.

I ſeize likewiſe this opportunity of recommending to the conſideration and trial of the faculty, the new neutral ſalt indicated in my VIIIth Experiment; both as it is a new and untried ſaline compound, and as much benefit may be expected from it, even *a priori*, from the known properties and activity of fixed air, largely introduced into the ſyſtem; particularly as a febrifuge and antiſeptic, in fevers, and other diſorders,

ders, of a putrid tendency.—As to the preparation of it—though I had before found that in proportion as the alcaline ſalt approaches to the ſtate of neutraliſation, it attracts the mephitic acid more weakly; yet ſince I wrote the preceding letter, I have prepared near 3 pints of a neutral ſolution of this kind, in Dr. Nooth's apparatus, as improved by Mr. Parker, which contained 10 grains of ſalt of tartar in each ounce of water. Notwithſtanding the unavoidable diſſipation of the fixed air in the upper veſſel, the alcaline ſolution was rendered perfectly neutral in about 24 hours, in conſequence of frequent agitation, and the ſucceſſive addition of freſh portions of fixed air; and, after ſuffering it to ſtand two or three days longer, it became pleaſant to the taſte, ſtrongly acidulous, and even pungent. My acquaintance with this neutral julep is of too late a date to enable me to add any thing material to what I have before ſaid of it. The preſent, indeed, is not a fit ſeaſon to quaff large potations of cold water, by way of experiment.

Nov. 29, 1775.

ERRATA.

PAGE 27, line 12 read, *was conſiderably injured by it*; leaving out the Remainder of the Paragraph, in which there is ſome Errror, that I cannot rectify without repeating the Experiment.

P. 41, l. 8, for *redreſs* r. *redneſs*.

P. 49, l. 4, for *common* r. *fixed*.

P. 88, l. 21, for *air*, r. *air*;

P. 315, l. 21, for *injured air* r. *injured*.

THE

THE

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To both of the VOLUMES.

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